A

DISSERTATION

ON THE

Senſible and Irritable Parts

OF

ANIMALS.

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Tranſlated from the LATIN.

With a PREFACE by M. TISSOT, M. D.

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DR. TISSOT'S

PREFACE

TO THE

Lauſanne Edition of this Treatiſe.

AS long as philoſophy was confined to hypotheſes founded only upon imaginary facts, this ſcience might be conſidered as a downright Proteus, putting on new forms almoſt every day, becauſe one fanciful opinion has always a right to ſupplant another. Hence it happened, that nature remained abſolutely unknown, and the beſt philoſopher was only a man of a tenacious memory, who had ſtuffed it with chimeras of all thoſe who went before him, and either gave the preference to ſome particular one, or rejected them altogether to make room for his own. During the laſt century, ſome happy geniuſes, the chief of whom we may reckon the Lord Chancellor BACON, diſcovered the fallacy of this ſort of philoſophy. They found it abſolutely ne-

ceſſary to reject all that chimerical nonſenſe which paſſed by the name of philoſophy; and they were ſenſible at the ſame time, that this ſcience was not to be created, but ſtudied. That they muſt obſerve the phænomena, which is natural hiſtory, and experimental philoſophy; and enquire into the cauſes of them; which is rational philoſophy, and, properly underſtood, is no more than obſervation, but very accurate and extended. This comprehends at once a great number of phænomena, obſerves what they have in common, and how they are connected; and not contented with the phænomena themſelves, it endeavours to penetrate into the cauſes of them, to unravel the properties of the matter which produces them, to diſcover the principal ones which ſerve as cauſes to a great number of others, and which may properly enough be called *the keys of nature*; for the knowledge of them actually ſolves a number of facts, of which we did not diſcover the reaſon before; and a fact, of which we are acquainted with the cauſe, is a great deal more intereſting and uſeful than another. It appears very obvious, that this rational philoſophy cannot make ſuch rapid progreſs as natural hiſtory; in the mean while it has advanced from time to time. The properties of the air, the circulation of the blood, and electricity,

tricity, which were diſcovered in leſs than the ſpace of a century, have thrown more light upon philoſophy than it had received for two thouſand years before, and they have ſucceſſively attracted the attention of all the philoſophers in Europe. The great diſcovery of the preſent age is IRRITABILITY, deſcribed in the following treatiſe, which I ſhall not pretend to ſay any thing in praiſe of, ſeeing its celebrated author, for theſe twenty years paſt, has favoured the public with ſo many excellent performances, and now this ſubject is become the principal topic of all thoſe who devote themſelves to the important ſtudy of the animal œconomy.

This diſcovery has met with oppoſition from different motives. Indolence, to avoid the trouble of an examination; vanity, to ſhun the imputation of ignorance; and envy, to deprive the diſcoverer of his due praiſe, have denied its exiſtence; and after it was confirmed by ſo great a number of facts, that it was impoſſible for the ſtrongeſt prepoſſeſſion to call the truth of it in queſtion, they have attempted to prove that it was diſcovered before, under other properties which have been known a long time. But this laſt reſort was very ſoon given up. *Irritability* is a property entirely different from all thoſe which were known before in the

dy *; and being eſſential to all animals, perhaps likewiſe to all plants, will henceforth juſtly be reckoned amongſt the principal qualities of organized bodies.

It muſt appear very ſurpriſing, and at the ſame time not a little mortifying to mankind, that a property which, as Dr. ZIMMERMAN ſays, conſtitutes perhaps the very baſis of life, and which chance muſt have made us ſenſible of a thouſand times, ſhould have eſcaped the eyes of all who imagined themſelves to be obſervers, and ſome of whom were actually ſuch. Perhaps it might not be impoſſible to aſſign the reaſon of this phænomenon, if the time for it was arrived; but it is ſufficient to ſay, that it reſembles ſeveral others of the ſame kind. Attraction, the weight and elaſticity of the air, ſhewed themſelves to the ſenſes every day; but it required a TORICELLI and a NEWTON to illuſtrate them. Why may it not be the ſame caſe with Irritability? and may it not be juſtly ſaid, that diſcoveries of this nature do ſo much the more honour to him that makes them, that he lives in a more enlightened age? When we are unacquainted with a thing we examine it cloſely, and in an ob-

* Vis ab omni aliâ hactenus cognitâ proprietate corporum diverſa & nova eſt: neque enim a pondere, neque ab attractione, neque ab elatere pendet. *Prim. Lineæ Phyſiol.* § 408.

ject which no body has examined before, we expect to meet with ſomething new; but in an age like the preſent, and upon a ſubject ſo much ſearched into as the human body, we do not flatter ourſelves with diſcovering eſſential properties; all that we can naturally hope for, is only to puſh farther thoſe diſcoveries already made, which require no more than dexterity and patience to bring them to perfection. Wherefore to diſcover ſuch a property as Irritability, and to examine it after it is diſcovered, requires a penetrating accurate genius, a lively ſenſe of the want of ſuch a diſcovery, and a ſagacity of *ſmelling it out beforehand* ſo to ſpeak. Such a perſon ought to know perfectly all that was known before, and repreſent it to himſelf in the ſtrongeſt light, in order to be convinced that he does not ſee falſely, or obſerve what others have taken notice of before, but have not judged worthy of their conſideration. He ought to have a particular turn to diſcovering the truth, and a ſtrong propenſity to lay hold of it, not to ſuffer this firſt glimpſe to eſcape him, which to common eyes would appear only an *ignis fatuus* not worth attending to, but ſhines like a bright meteor to thoſe whom nature has deſtined to obſerve it; which method of diſcovering is in a manner to create. But may Dr. HALLER

be ſaid in this ſenſe really to have created Irritability? You will find at the end of the following treatiſe a ſhort hiſtory of this quality; in which he informs us, that ſeveral men of learning have given him the honour of the diſcovery; but far from ſubſcribing to their teſtimony, his modeſty, which is always a mark of ſuperior talents, prompts him to name ſome authors, in whoſe works he alledges, it was hinted at before. But if you only take the trouble to read them, you will eaſily ſee, that what is contained in them proves only that they imagined a concealed cauſe, to which they attributed thoſe phænomena which they could not account for; but by no means that they were acquainted with Irritability. HIPPOCRATES long ago ſearched after this cauſe, and pointed it out by the name of ἐνορμον; and BAGLIVI, who owns himſelf that his ſyſtem was founded upon the ideas of HIPPOCRATES, imagined a force in the ſolids which he did not diſtinguiſh from other known forces, which he evidently appears to confound with elaſticity, and places it in parts where it is not to be found. GLISSON is the firſt, ſays Dr. HALLER, who made uſe of the word *Irritability*, but in reality GLISSON ſaw no more than the common butchers ſee every day, *viz.* palpitations of the fleſh after death, which are

renewed

renewed upon touching. The tone of the fibres, ſo often repeated by the followers of STHAL, is no more than elaſticity; and the principle of motion of Dr. GORTER, is only elaſticity joined to ſenſibility. Nay, this able phyſician has made ſo few experiments upon the ſubject, that he attributes the cauſe of fevers * to the irritation of the arteries, which are neither irritable nor ſenſible. This however was all that was wrote upon the ſubject, when Dr. HALLER, in 1739, publiſhed his firſt notions of Irritability, which he continued to explain in the following years. Some years after Dr. WINTER, in an academical oration, and likewiſe in a theſis publiſhed by one of his pupils, founded a ſyſtem upon Irritability, ſuch as he imagined it to be, and not at all ſuch as it really is. In ſhort, it appears from thence, that far from being the inventor of it, it may juſtly be ſaid, that he had not a true idea of it. His ſyſtem is the ſame with that of BAGLIVI, and ſo little corrected, that he has fallen into the very ſame error with him, taking for the principle of all our motions the *dura mater*, which is void of all ſenſation. What dependence can we have upon ſyſtems purely imaginary, and the futility of which is

* Compendium Tr. 52. § 9.

proved by one ſingle experiment? But at the ſame time Dr. WINTER is very much to be commended, for having publicly acknowledged that he was in a miſtake, though certainly ſuch an acknowledgment muſt be leſs mortifying to a man of his ſuperior reputation, than to thoſe authors of low rank, who are only known by publiſhing falſe doctrines, and ſoon relapſe together with their works into oblivion. Dr. KAAU BOERHAAVE in his ingenious treatiſe *de Impetum faciente*, publiſhed only in 1745, makes obſervations on the ἐνορμον of HIPPOCRATES, but that he has not placed it in Irritability, appears from the manner in which he characteriſes it §145. He ſays it is a force which belongs neither to the body nor the ſoul, which takes place the moment theſe two are united, and ceaſes the very moment of their ſeparation. But this character is very oppoſite to that of Irritability; chimera and reality are not to be confounded together. If he has mentioned this property in his other works which are ſince publiſhed, it is eaſy to ſee from what ſource he has derived it, and we ought to conclude, that Dr. HALLER is really the perſon who has diſcovered and explained the doctrine of Irritability. The confuſed hints which are to be met with elſewhere, ought no more to deprive him of the honour of this diſcovery, than the

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ſympathies of ARISTOTLE, or the obſcure and univerſally extended force of BACON Lord VERULAM, ought to deprive Sir ISAAC NEWTON of that of having firſt known the force of attraction; and as this property will be tranſmitted to the lateſt poſterity under his name, ſo Irritability will be diſtinguiſhed for the future by the epithet of HALLERIAN. Thoſe obſcure and falſe ideas which are to be met with in ſome works, have been ſo far from facilitating Dr. HALLER's diſcovery, that they ſhould rather have tended to obſtruct it. In the arts an imperfect ſketch, or even a faulty one is of uſe, as it leads to ſomething better, and by degrees to perfection. We owe to the rudeſt draughts thoſe very machines which at preſent attract our admiration; and the firſt hut may be conſidered as having been the model of the moſt lofty edifices. But the caſe is by no means the ſame in the ſciences; a faulty ſyſtem, eſpecially if it is founded on fantaſtical ideas, impoſed upon us inſtead of accurate experiments, leads us aſtray, renders more difficult, retards, and frequently abſolutely obſtructs the progreſs which we might otherwiſe be able to make; and we ought to reckon ourſelves much more obliged to thoſe, who, in order to come at the truth, are obliged to eradicate a thouſand errors, which are planted like ſo many obſta-

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cles in the way, than to thoſe who find out a ſmooth road, though it has not been beaten before, and this obligation we lay under to Dr. HALLER; becauſe in order to diſcover the true principle of motion in man, he has been obliged to make his way through a whole heap of imaginary ſyſtems.

The whole animal œconomy rolling upon this principle, it is eaſy to imagine what a change this diſcovery muſt produce, in explaining of facts. To England we owe philoſophy, and to Switzerland we ſhall owe phyſiology, the immoveable baſis of which will be the treatiſe on Irritability. We may ſee by this ſpecimen of our author, the happy uſe which he has already made of that property.

A great number of facts upon which Irritability is eſtabliſh'd, were formerly publiſh'd by Meſſieurs ZIMMERMAN, OEDER, CASTEL, ZINN, SPROEGEL and WALSTORF, pupils of Dr. HALLER, witneſſes of thoſe experiments, encouraged by his precepts, and animated by his example to make new ones themſelves. Their works are known, and have been well received by the public, as they juſtly deſerv'd; but in all of them there wanted that laſt degree of preciſion, which is never found in the firſt eſſays upon a ſubject intirely new, which is only the production of a maſterly hand, and characteriſes the following

following treatiſe, inſerted in Latin in the ſecond volume of the *Tranſactions of the Royal Society of Gottingen.* In this treatiſe we find a very careful diſtinction between Irritability and Senſibility; experiments made with an exactneſs, of which thoſe who are uncapable of the ſame, are neither ſenſible of the difficulty nor the uſe; determining the parts which are ſuſceptible of the one and not of the other, thoſe which poſſeſs neither the one nor the other, and thoſe in which both are united together. As I imagined that a table, to repreſent at one view the reſult of all theſe experiments, would be of ſome ſervice, I have inſerted one at the end of this preface.

Perſons of inferior capacity, to whom nature has left no other means of aſſociating their names with thoſe of great men, but by vilifying their works, and who deny the uſefulneſs of theory in practice, becauſe they do not comprehend it, will perhaps aſk what is the uſe of all theſe diſcoveries, and if the art of healing will receive any new degree of perfection from them. Such perſons do not conceive the uſe of theory in practice, for want of that thorough knowledge of both, which is neceſſary to diſcover the connexion between them, and that extent of genius, which, taking in ſeveral objects at the ſame time, and uniting them together

ther in one point of view, discovers the connexion that there is between them, and the necessary relation which obtains in all the sciences, between that of knowing the human body and the art of healing. Consult those celebrated physicians whom all Europe look upon as the first of the present age (I might have said as eminent as ever appeared) *viz.* VAN SWIETEN, WERLHOF, TRONCHIN, ELLER, SWENKE and DE HAEN, they will all tell you, that they owe the extraordinary and continued success, which has raised their reputation to so high a pitch, *to this enlightning theory*, as M. DE LA METTRIE says, speaking of that of the great BOERHAAVE, *which alone would be sufficient to persons of the least experience, and direct them safe in their practice, whilst without it the most consummate practitioner must always continue reduced to guess and conjecture.* One may say, that great physicians have a different practice from those of the ordinary sort. The former have particular methods of treating their patients, which the latter cannot comprehend the reason of, because they depend upon a skilful application of general principles, which these are either entirely ignorant of, or else are not capable of making a proper use of. They follow slavishly the same beaten method, though it has frequently been found

found hurtful, or at leaſt uſeleſs; and as they are incapable of leaving it, all that we can expect of them is, that they ſhould ſucceed in caſes where that method happens to be proper. You muſt not require any thing further of them, it is very well if, in order to conceal their ignorance, they do not run down thoſe preſcriptions, which are above their comprehenſion as to their uſe, and over which ſhould be inſcribed the following motto,

Odi prophanum vulgus.

If the dependence of pathology on phyſiology was better known, there would be no need for diſplaying the great influence which this new diſcovery muſt have upon the art of phyſic; but unfortunately we want a work intitled, *The application of theory to practice.* This has determined me to venture ſome thoughts on the practical uſes of Irritability, which perhaps may ſerve to incite the curioſity of the reader to further diſcoveries *.

* We have already two theſes in which the cure of Irritability in practice has been treated of, the one is that of MANITIUS *de idioſyncraſia ex diverſa ſolidorum corporis humani irritabilitate optimè dijudicanda*; and the other of Dr. DE LA MOTTE of Paris, *an omnis morbus ex irritabilitate aucta aut imminuta*; but theſe two works have left the ſubject ſtill new.

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The manner in which opium acts, which has given rise to so many systems equally opposite and chimerical, and has occasioned such a number of disputes, which could never before be decided, is determined at last, since the discovery of Irritability. It is neither by attenuating nor by thickening the humours, by exalting or absorbing the sulphureous particles, by restraining the furious *archeus*, nor by binding the nervous fluid, that opium occasions sleep; but by diminishing the Irritability of all the parts, excepting that of the heart, which is only a very little, and most frequently not at all weakened by this medicine. The action of the muscles intirely ceases; the senses are buried in a profound sleep; the heart only and the lungs, the one because its Irritability is not at all altered, the other because its action is independent on Irritability; I say, these organs alone continue their motions the same as before. Those viscera which are in the same case with the lungs continue their functions, while the actions of the stomach and intestines are diminished, and hence may be deduced, in what cases opium must be serviceable in stopping too violent evacuations, *viz.* when they depend upon too great an Irritability of the intestines; and consequently if this is too weak, narcotics must be hurtful.

This great principle ſerves as a baſis to the whole practice of this remedy, and the manner of its acting accounts for all the ſymptoms which it produces. But it would be too tedious to enter into this detail, which every one may eaſily follow.

We frequently ſee perſons in whom the moſt trifling moving cauſe, occaſions much more conſiderable commotions, than it produces in thoſe of a ſtronger habit. They cannot bear the ſlighteſt unuſual impreſſion; the leaſt alarm, from whatever cauſe, will occaſion extraordinary ſymptoms in them, which, according to their appearances and the part where the firſt cauſe of the complaint is imagined to reſide, are known by the name of vapours, or hyſterics, or other extraordinary complaints*, the immediate cauſe of which has always been attributed to an exceſſive mobility of the animal ſpirits, but the true one is too great an Irritability of the parts. This principle, combined with ſenſibility, accounts for the moſt extraordinary phænomena attending thoſe

* The celebrated Dr. Gorter, to whom the practice of phyſic is ſo much obliged, is the firſt who has treated expreſsly of *mobility*, a diſeaſe ſo frequent and ſo little known. The deſcription which he gives of it is very exact, and I would adviſe every phyſician to be acquainted with what he ſays of it in his *Compendium*, and his *Syſtema Praxeos*.

diſor-

diſorders, and at the ſame time directs us to the proper cure of them. In ſhort, ſeeing Irritability depends upon the mucus, and the different degrees of it are proportioned to the conſiſtence of this ſingular body, ſo that the former is greater in proportion as the latter is leſs *, in order to remove the exceſs, the mucus muſt be reſtored to its neceſſary conſiſtence. Thoſe remedies therefore which are called tonics are the only ones to be made uſe of for this purpoſe; bleeding, purging, ſalts, mineral waters (at leaſt the moſt part of them), and watery medicines, ought to be forbid, and in place of them ſhould be ſubſtituted a proper regimen, exerciſe, friction, ligatures, gentle aſtringents, aromatic wines, *&c.* And indeed practice having ſo often confirmed the uſefulneſs of this method, may we not hence juſtly conclude that ſyſtem which explains them to be true, and which Dr. HALLER only propoſed by way of conjecture? Age, which gives a greater conſiſtence to the mucus, diminiſhes this exceſſive mobility; and hence we ſee inſtances every day of hyſteric women getting rid of theſe complaints, or at leaſt becoming a great deal leſs ſubject to them, after a certain age. There is likewiſe a point beyond which the

* Dr. ZIMMERMAN p. 8.

conſiſtence

conſiſtence of the mucus becomes bad by growing too thick, becauſe hereby the Irritability is rendered too weak to allow the motions to be performed by their ordinary cauſes; and this thickneſs being the inevitable conſequence of old age, hence this ſtate is neceſſarily followed by death, which is nothing elſe than a ceſſation of all motion; for in old age Irritability gradually ceaſes, and without Irritability there is an end of motion, and without motion no more life. Nature produces in the tendons the effect of old age, and though they are compoſed of muſcular fibres, and are really a continuation of the muſcles, their too great compactneſs prevents their being irritable. This very phænomenon, if it was thoroughly examined, might perhaps ſerve to inform us in what the Irritability of the mucus conſiſts; the explications which I have juſt hinted at ſupply us with others for a great number of phænomena, and lead us to the true rules of practice in a great many caſes, where till now they were extremely falſe.

The cauſes and cure of convulſive diſorders, ſo intimately connected with hyſterics, receive a new light from this diſcovery; ſpecifics are hereby rejected as uſeleſs, and there are only two indications to be anſwered, *viz.* to remove the ſtimulus and diminiſh the Irri-

tability. But evacuating medicines always encreaſe this, and therefore can only be of ſervice in caſes where they are capable of removing the ſtimulus.

The action of thoſe purgatives which we are better acquainted with, aſſiſt us in determining the uſe and choice of them. Diſeaſes of the bowels, the cure of which is ſometimes ſo tedious and difficult, that able phyſicians have looked upon them as incurable after they are become inveterate, will be removed with more eaſe, becauſe their cauſe being known we are thereby led to find out their remedies. Chance has diſcovered, that perſons who have been drowned have ſometimes been brought to life again by air being blown into their anus. Reaſon teaches us that this effect muſt be produced by exciting the Irritability of the inteſtines, which renews that of the vital organs; and hence it is concluded, that an irritant as harmleſs but more powerful than air, ſuch as cold water, will produce more certainly the ſame effect. It is eaſy to conceive how remedies may act when all ſenſation is loſt, after we know that the organs of motion and ſenſation are not the ſame. We may ſee in Dr. ZIMMERMAN's eſſay* the manner in which he explains that phænomenon, which before

* § 39.

was

was not to be accounted for, *viz.* why ſome paralytics preſerve the ſenſe of feeling, whilſt others who are called *pareſie*, loſe the ſenſe of feeling and preſerve that of motion. Palpitations are now eaſily to be explained, and for the honour of all the pathologiſts who have ſearched into the cauſe of them, it were much to be wiſhed that Irritability had been ſooner diſcovered. By diſpoſſeſſing ſeveral parts of the melancholy right which was aſcribed to them, of being the ſeat of pain, and by marking out thoſe which are really ſo, Dr. HALLER teaches us to which we are to make our applications, and thereby renders perfect the art of phyſic in one very important part, *viz.* that of quieting the ſymptoms.

The theory of the conſtitutions explained by Irritability, in a work which Dr. ZIMMERMAN is preparing upon this ſubject, will throw a new light upon the whole practice and baſis of morality. The influence of our body upon our ideas is ſo ſenſible, that every one is acquainted with it. We ſee it verified every day, that a ſmall quantity, more or leſs, of food, a few drops of certain liquors, or ſome grains of nightſhade, intirely change our manner of ſeeing things, and conſequently of judging of them. Our ideas of beauty and goodneſs, of good and evil, or of vice and virtue, and our

actions conſequent upon theſe ideas, vary according as our blood circulates with more or leſs rapidity, or is more or leſs thick. It is therefore certain that our manner of living changes our manner of thinking, that the operations of the mind while it is united with the body, may be varied by the uſe of air, diet, old age, ſleep, motion, reſt, and medicines. In conſequence of this there muſt be a medicine for the mind, which has been thought of in all ages; in all ages it has been wiſhed that this ſubject was treated of, that the true principles of it were enquired into, and certain practical precepts of it laid down. But this work has never been brought to perfection hitherto; all that we have, even the moſt modern, upon this ſubject, proves the difficulty of the enterprize and the boldneſs of the undertakers, much more than their capacity. In a work of this kind ſuch univerſal knowledge is required, that it is by no means ſurprizing it has hitherto been wanted; but this eſſential vacancy in the libraries of moraliſts and phyſicians, will be filled up with dignity by Dr. ZIMMERMAN'S treatiſe, which obligation we ſhall owe to Irritability.

There will be no further occaſion to have recourſe to imaginary ſuppoſitions, in order to explain the phænomena of the apoplexy. If the

the heart and other organs of the circulation continue their motions, when all the animal motions are ſuſpended, 'tis owing to the ſame reaſon that explains the action of opium; *viz.* becauſe there is a ſtimulus which determines the motion of the heart, independent of all ſenſation and of every other motion. But the apoplexy is only a profound ſleep, depending upon the very ſame cauſes with ſleep, and therefore is to be accounted for in the ſame manner *.

The theory of fevers, and that of inflammations, in a word, of all the diſeaſes which depend upon an increaſe of the circulation, will henceforward be fixed; ſeeing the cauſe of the circulation being known, we are thereby conducted to the knowledge of thoſe things which can either increaſe or weaken it. The blood being rendered more acrid, of courſe becomes more irritating, hence acrimony is capable of producing a fever; and the different ſpecies of acrimony, the order in which they are produced, or evacuated, will form the different ſpecies of fevers. There ſtill remain diſcoveries to be made upon Irritability, eſpecially with relation to the force of different ſtimulants, which depends perhaps upon ſeveral cauſes;

* See the *Primæ Lineæ Phyſiologicæ*, N° 568, 576, and 403.

and the more that the nature of it comes to be underſtood, the more eaſy will it be to give a reaſon for all the motions depending upon it.

Several accidents in ſurgery, which were only troubleſome becauſe we were miſtaken about the cauſe of them, will henceforth ceaſe to be ſo, ſeeing their cauſe being better known leads us to the proper method of treating them, and the method of treating them being known, aſſures us of a cure *. The uncertainty which ſurgeons apprehended of the poſſibility of ſeveral important operations, and which men eminent in the profeſſion, ventured upon only as deſperate, while others durſt not at all meddle with them, has occaſioned the death of a number of perſons, in caſes where the patients will be ſaved for the future, becauſe theſe new experiments aſſure us of the ſafety of thoſe operations.

The examples which I have already related, will be ſufficient, I hope, to convince perſons of the greateſt obſtinacy, of the real advantages which the diſcovery of Irritability muſt procure to us. I ſhall finiſh with ſome general reflections upon the objections which may be made or have already been made to it.

* See Dr. ZIMMERMAN p. 14, 15 and 16, and Dr. CASTELL § 42, 43, 44 and 45.

1. It is by no means an ideal ſyſtem which Dr. HALLER advances in his treatiſe, it is not an aſſemblage of analogical concluſions upon four or five experiments haſtily made, and frequently in ſo bad a manner, that the chief care of the author is to reconcile the reſults of them; but it is a chain of facts, which have been confirmed by a number of experiments made with the greateſt exactneſs, and repeated very frequently during the courſe of ſix years, before the publication of this treatiſe, and even ſince that till the preſent time; the conſequences of which have always been uniform, and have all concurred to confirm the ſame truth. It is not therefore by a few arguments that Irritability is to be attacked; it is not by trivial objections, founded upon chimerical conſequences, which an over-heated imagination may draw from them; neither is it by ſome obſervations, or ſome experiments made at random. Whoever wants to deny the facts which Dr. HALLER advances, or rather that his experiments have been properly made, ought to be as well provided as he with a number of experiments equally well atteſted. But it is not to be ſuſpected that Irritability will ever be attacked in this manner; to believe this would be wronging of nature, which is invariable in her laws, as thoſe who know them by inquir-

ing into them will always find her. When obſervations upon the ſame ſubject do not reſemble each other, it is always either becauſe one of the obſervers has not perceived the different circumſtances which ought neceſſarily to vary them; or becauſe, as indeed is too often the caſe, he decides the reſult of the experiment before he has made it, and only makes it in order to confirm the other; ſo that he ſees what he was reſolved beforehand to ſee. Some phyſicians treat the book of nature as the divines have treated the bible; they do not conſult it in order to know what it contains, but to find in it ſomething to authorize their ideas. They do not apply to nature for information, but they feign oracles, and deliver them boldly as her deciſions. Thus books are multiplied, and confuſions in proportion, becauſe the falſe muſt be lopped off before the truth can be laid hold of; and I ſhould not be much ſurprized, if a perſon who knew nothing of the univerſe but by the works of ill qualified or ſyſtematic authors, (who are by far the moſt numerous) ſhould believe it to be the work of chance, ſo little uniformity and harmony would they find there.

2. The experiments relating to Irritability having been made upon brutes, can the truth of the reſult of them be affirmed with regard to man? It is eaſy to ſee that this objection

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is the effect of that low jealouſy, which perſecutes talents and merit, or rather mankind in general, by endeavouring to diſcourage thoſe great men who enlighten it; provided they were capable of being hurt by thoſe weapons, which, like ſlender arrows drawn by the weak arms of children, can only riſe high enough to fall back upon their own heads. But we muſt not leave even this gloomy conſolation to envy; while we deſpiſe the inſect which teaſes us, and which we do not diſtinguiſh from numbers of the ſame kind, we endeavour to guard ourſelves from its ſting, the effect of which is ſo much the more ſenſible, the handſomer the face is againſt which it is darted. Dr. HALLER's treatiſe conſiſts of two parts, the firſt of which treats of ſenſibility, and the experiments which he relates, contrary to what has hitherto been generally believed, are ſuch as we had the greateſt reaſon to ſuſpect to be inapplicable to man. But the human body has really been the ſubject of moſt of theſe experiments, and hence all thoſe doubts neceſſarily ceaſe. He mentions ſome authors who had obſerved before him the inſenſibility of the tendons, and he proves that inſenſibility, by a fact which he was witneſs to himſelf. In the ſupplement he quotes the celebrated Dr. ELLER, whoſe authority cannot be ſuſpected, as a witneſs of the inſenſibility

ſibility of the *dura mater*; and Dr. CASTELL relates other facts which prove the ſame thing *. It is true we have not the ſame number of experiments upon the Irritability of the human body as we have upon that of brutes, but ſtill we have ſome; and ſuppoſing there were none at all, it would be equally juſt to conclude from the ſevereſt analogy that it exiſts. That pyrrhoniſm which denies all certainty, is not more fooliſh and dangerous, than that which admits only of geometrical demonſtration. Inferences have their rules, and thoſe propoſitions which we diſcover in following them exactly, have the ſame degree of force, as mathematical propoſitions the moſt ſtrictly demonſtrated. They are not to be conteſted, but from jealous ignorance, which is always inconſequential for want of principles. The moſt part of thoſe phyſiological experiments, which during this laſt century have advanced phyſic to its preſent height, have been made upon brutes; and it is to theſe experiments that we owe the knowledge of the circulation, the mechaniſm of reſpiration, the rout of the chyle, and the hiſtory of generation. But there have never been any objections raiſed againſt their application to the human phyſiology; becauſe

* Pag. 23, 24, 25 and 38.

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we cannot be deceived as to the perfect uniformity of the mechaniſm in us and brutes, with regard to the vital and natural functions; for that is demonſtrated by the exact reſemblance of ſimilar parts, and of the eſſential organical parts. The different make of the extremities, or the variety of the outward ſkin, does not prove the principles of their motions to be different. A crane that lifts a beam, or a block of marble, is always the ſame crane, and acts in the ſame manner in both caſes. We may therefore conclude that Irritability in man is one of thoſe truths which is undeniably demonſtrated; and poſterity, which alone ſtamps the merit of diſcoveries, by abſtracting of perſons, will promote this to that rank which its uſefulneſs intitles it to. She will laugh when ſhe obſerves, that after its oppoſers had failed in perſuading us there was no ſuch thing, they ſhould endeavour to render the doctrine odious, by the conſequences which they pretend naturally follow it. She will be diverted to ſee phyſicians following the example of religious ſectaries and devotees, intereſting the cauſe of God with theirs, and accuſing of deiſm ſuch as differ from them in opinion as to the pulſations of the arteries. A certain author, well known for the greatneſs of his talents and the bad uſe which he has made of them,

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has mixed in the ſame work ſome ideas of Irritability, with thoſe of materialiſm, and endeavoured to explain the ſenſations by this property; but Dr. HALLER, at the end of his treatiſe, has proved the futility of this ſyſtem. However as this objection is inſerted very carefully in a ſmall diſſertation of M. DELIUS, profeſſor at ERLANG *, and he goes ſo far (ſo charitable is his doctrine) as to endeavour to prove ſyllogiſtically, that this new ſyſtem tends to irreligion, therefore this propoſition deſerves to be examined.

1. It is generally allowed that the nerves are the organ, and the brain the receptacle of all our ſenſations, the ſources of all our ideas; but the nerves and the brain are not at all irritable; Irritability therefore has nothing common with our ſenſations. 2. Suppoſing it was affirmed to be the principle of our ſenſations, as it appears to be that of the other motions, what dangerous concluſion could hence be deduced? What does it ſignify to thoſe truths which depend on the nature of the ſoul, whether that be Irritability or ſome other property of matter? That analogy which I have proved before between men and brutes, (I mean all this time quadrupeds) clearly demonſtrates

* Animadverſiones in doctrinam de irritabilitate, tono, ſenſatione, & motu corporis humani.

that

that the principle of the ſenſations is the ſame in the one as it is in the others, and this principle not being the ſoul in brutes, can with as little propriety be called the ſoul in man. Senſation is produced the ſame way in the one as in the others; but in brutes, the reſult of this ſenſation is confined to a mechanical determination conſequent upon it; while in man the ſoul perceives that ſenſation, this perception forms the idea, and the paſſage from the ſenſation to the idea is the eſſential character which diſtinguiſhes man from brutes. This difference, which ſo many divines deny, that they may enjoy the gloomy pleaſure of depreſſing man below the brute creation, and of diſcovering leſs reaſon, wiſdom, and conduct in him than in them; this difference, I ſay, has been lately ſhewn in as clear a light as poſſible, and that principle upon which deiſm founded one of its ſtrongeſt arguments, has thereby been undermined. That induſtry, wiſdom, foreſight, knowledge, all theſe wonders, I ſhould rather ſay all theſe monſters of reaſoning, creatures of the imagination of the obſervers, and of that deſire which they have of diſcovering every where the final cauſes of things, fruits of that vanity which attempts to explain every thing, and of that want of capacity which conſiders thoſe things as trifling which only exiſt to give

harmony to the whole; all theſe chimeras vaniſh, and if beings, intirely corporeal, perform their labours with more order than men, it is only becauſe matter, conducted by the Creator, is better managed than that which is in the hands of the creature. The brutes, properly ſo called, are reſtrained by wiſe laws, which in them are invariably executed; whereas, on the contrary, the ſoul frequently rebels againſt theſe laws, in man. From all theſe facts the following ſyllogiſm reſults, very oppoſite to that of profeſſor DELIUS at Erlang, *viz.* a property common to two beings is by no means the cauſe of their difference; but becauſe Irritability is common to man and brutes, it is not therefore the cauſe of thought. It influences both the vital and natural motions, nay further, it may be allowed to influence the ſenſations, and all the animal motions depending upon them, without this doctrine being ſuſpected of impiety, becauſe we are certain that the cauſe of ſenſation is independent of thought. Perhaps the ſoul may abſent itſelf from the body, or, to ſpeak more juſtly, may take no notice of what paſſes, without the life being thereby affected in man. What employment could the ſoul have preſiding in the wild man of *Arnobius*, if ever there was ſuch a perſon? What employment

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can it have in the fœtus, that maſs indeed organiſed, but deprived of all ſenſe, and buried in perpetual ſleep? Does it ſhew any ſign of its preſence in a new born infant? One is loſt in chimerical queſtions concerning the moment of the union of the ſoul and body, which is doubtleſs not ſingle; for the body can live without the ſoul. This union only conſiſts in the inſpection which the ſoul has over the body, and does not take place but when this inſpection is exerciſed, and the ſoul of conſequence produces ſome motion in the body. During the firſt months of our life, this union is far from being continued, but it becomes more ſo by degrees, though perhaps during life it has its interruptions, which are probably the cauſe of theſe contrarieties, the reaſon of which has not hitherto been diſcovered.

We are but imperfectly yet acquainted with the phænomena of the load-ſtone, of attraction and electricity. Irritability comes next to open a new field of enquiries, a new ſource of ſolutions; and perhaps we are upon the point of diſcovering ſome other property, that will throw a light upon thoſe obſcure ſubjects, of which at preſent we ſee only the dawn.

The ſenſible parts.

THE brain, the nerves, *viz.* their *medulla*, and the following parts by means of the nerves.

The ſkin, the muſcles, the internal membrane of the ſtomach, inteſtines and bladder, the ureters, the breaſts, the *uterus*, the *vagina*, the *penis*, the tongue, the teeth, the eyes, and eſpecially the *retina*, the *tunica choroides*, but leſs than the *retina*; the heart, though not ſo much as the other muſcles.

The *viſcera*, the lungs, liver, ſpleen and kidnies, as likewiſe the glands, have few nerves beſtowed upon them, and conſequently are not endowed with much ſenſation.

The inſenſible parts.

THE cuticle, the cellular membrane, the fat, the tendons, the membranes which cover the viſcera and articulations, the *mediaſtinum*, *omentum* and meſentery; the *dura* and *pia mater*, the ligaments, the *periоſteum* and *pericranium*, the bones, the marrow, the *cornea* and *iris*.

The arteries and veins are inſenſible, excepting thoſe which are accompanied with nerves, ſuch as the carotid, the lingual, the temporal, the pharyngal, the labial, the thyroidal, and the *aorta* near the heart.

The cornea, Havers's gland, and the coats of the nerves are likewiſe inſenſible.

The irritable parts.

The heart, the muſcles, the diaphragm, the *œſophagus*, the ſtomach and inteſtines, the lacteal veſſels, the thoracic duct, the bladder, the glands and mucous ſinuſes in man, the *uterus* and genitals, which laſt are endowed with an Irritability peculiar to themſelves.

The unirritable parts.

The nerves, the cuticle and the ſkin, the membranes, the arteries, the veins, the cellular membrane, the *urethra*, ureters, lungs, liver, ſpleen, *dartos*, and round ligaments of the *uterus*, the tendons, ligaments, *periоſteum*, *meninges* of the brain, and the *iris*.

The excretory ducts can ſcarely be called irritable, at leaſt they require a very ſtrong irritation.

The parts which are both ſenſible and irritable.

All thoſe which have nerves and muſcular fibres; the muſcles, the heart, the whole alimentary canal, the diaphragm, the bladder, the *uterus*, the *vagina* and genitals.

Inſects are irritable and ſenſible all over.

A

TREATISE

ON THE

Senſible and Irritable Parts

OF

ANIMALS.

SOME months ago [a] my pupil and friend Dr. ZIMMERMAN publiſhed an inaugural diſſertation upon Irritability; part of the experiments relating to that ſubject I ſaw him perform, and I ſhall relate them here, in the ſame order as I find them, in my notes. Others of them I was not witneſs to, and therefore I ſhall quote theſe from his theſis. Since the year 1746, I have made a great many of the ſame kind myſelf, which he was either a witneſs

[a] Dr. HALLER read this memoir in the academy of Goettingen, April 22, 1752, and the diſſertation which he quotes was publiſhed in July 1751, under the following title, *Diſſertatio Phyſiologica de Irritabilitate, authore* JOHANNE GEORGIO ZIMMERMAN Helveto Brugenſi.

of, or aſſiſted me in performing; and ſince the beginning of the year 1751, I have examined ſeveral different ways, a hundred and ninety animals, a ſpecies of cruelty for which I felt ſuch a reluctance, as could only be overcome by the deſire of contributing to the benefit of mankind, and excuſed by that motive which induces perſons of the moſt humane temper, to eat every day the fleſh of harmleſs animals without any ſcruple. As in making theſe experiments I was obliged to try ſeveral which were uſeleſs, and to repeat others of them ſeveral times, to communicate the whole of them would only be ſpinning out this treatiſe needleſsly; wherefore I ſhall confine myſelf to relate thoſe only which are of real uſe, and are found conſtantly true.

The reſult of all theſe experiments has given place to a new diviſion of the parts of the human body, which I ſhall follow in this ſhort eſſay, by diſtinguiſhing thoſe which are ſuſceptible of Irritability and Senſibility, from thoſe which are not. But the theory, why ſome parts of the human body are endowed with theſe properties, while others are not, I ſhall not at all meddle with. For I am perſuaded that the ſource of both lies concealed beyond the reach of the knife and microſcope, beyond which I do not chuſe to hazard many conjectures, as I have no deſire of teaching what I am ignorant of myſelf. For the vani-

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ty of attempting to guide others in paths where we find ourſelves in the dark, ſhews, in my humble opinion, the laſt degree of arrogance and ignorance.

I the more willingly engaged in examining this ſubject, that the experiments which I have made, are the ſource of a great many changes, both in the phyſiology, pathology and ſurgery, and diſcover ſeveral truths contrary to the opinions generally received. This laſt reaſon has obliged me to be very exact in my proofs, becauſe I was fully perſuaded, that an opinion ſo little foreſeen would at firſt appear improbable, and only gain aſſent by the cleareſt conviction. For this reaſon I was obliged to repeat and multiply my experiments, in order to convince the incredulous, by a number of authentic teſtimonies, ſo to ſpeak, and prevent my falling accidentally into any miſtake; for I am perſuaded that the great ſource of error in phyſic has been owing to phyſicians, at leaſt a great part of them, making few or no experiments, and ſubſtituting analogy inſtead of them. A ſecond motive which encouraged me in this work, was the readineſs with which ſome celebrated authors have laid hold of the firſt notions of Irritability, ſo as even to make uſe of this property of our fibres, as a baſis of almoſt an univerſal ſyſtem of motion in the human body, and thence deduce the functions of the fibres, veſſels, nerves, muſcles,

and in ſhort of all our organs. This appears very plain in reading Dr. WINTER's oration, delivered at Franeker in 1746, that of Dr. LUP's *De Irritabilitate*, and the theſis of DE MAGNI and LA MOTTE upon the following ſubject, *Ergo a vaſorum aucta aut diminuta irritabilitate omnis morbus.* For they all agree pretty much in the ſame opinion, *viz.* in deriving all motion from ſenſation: In which they are joined by Drs. KRUGER, NICOLAÏ, WHYTT, DELIUS, and ſome other great phyſiologiſts.

I call that part of the human body irritable, which becomes ſhorter upon being touched; very irritable if it contracts upon a ſlight touch, and the contrary if by a violent touch it contracts but little.

I call that a ſenſible part of the human body, which upon being touched tranſmits the impreſſion of it to the ſoul; and in brutes, in whom the exiſtence of a ſoul is not ſo clear, I call thoſe parts ſenſible, the Irritation of which occaſions evident ſigns of pain and diſquiet in the animal. On the contrary, I call that inſenſible, which being burnt, tore, pricked, or cut till it is quite deſtroyed, occaſions no ſign of pain nor convulſion, nor any ſort of change in the ſituation of the body. For it is very well known, that an animal, when it is in pain, endeavours to remove the part that ſuffers from the cauſe that hurts it; pulls back the leg if it is hurt, ſhakes the ſkin if it is pricked,

ed, and gives other evident ſigns by which we know that it ſuffers.

We ſee that experiments only can enable us to define what parts of the human body are ſenſible or irritable, and what the phyſiologiſts and phyſicians have ſaid upon theſe qualities, without having made experiments, has been the ſource of a great many errors, both in this caſe and in a number of others.

When Dr. BOERHAAVE had eſtabliſhed the doctrine of the nerves, being the baſis of all our ſolids, he preſently after proceeded to affirm, that there was no part of the human body which was not ſenſible, or capable of ſome ſort of motion[b], and this ſyſtem, which I have elſewhere[c] refuted, was received almoſt univerſally all over Europe.

The moſt ſimple parts of the human body are the nerves, arteries, veins, the ſmaller veſſels, membranes, the muſcular, tendinous, ligamentous, and bony fibres, and the cellular membrane.

The more compound parts are the muſcles, tendons, ligaments, bowels, glands, the great reſervoirs, excretory ducts, and the larger blood veſſels.

[b] Inſtitut. Med. N° 301. [c] *Commentar in* Prælect. Boerh. loc. cit.

But this by the by, for here I don't ſtudy accuracy, this being only a general liſt of the parts of the human body.

In order to diſcover which of theſe parts are ſenſible, I made the following experiments.

I took living animals of different kinds, and different ages, and after laying bare that part which I wanted to examine, I waited till the animal ceaſed to ſtruggle or complain; after which I irritated the part, by blowing, heat, ſpirit of wine, the ſcalpel, *lapis infinalis*, oil of vitriol, and butter of antimony. I examined attentively, whether upon touching, cutting, burning, or lacerating the part, the animal ſeemed diſquieted, made a noiſe, ſtruggled, or pulled back the wounded limb, if the part was convulſed, or if nothing of all this happened. The repeated events of thoſe experiments I marked down faithfully, whatever I found them to be. For what is it to me, in fact, on which ſide nature decides the queſtion? nay, would it not be very fooliſh to hazard the reputation of a faithful and accurate obſerver for an imaginary fact, which the ſimpleſt experiment would prove to be falſe to any other anatomiſt who ſhould chuſe to repeat it?

As to the order of making the experiments, I think it is of no great conſequence, and there-

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fore ſhall begin with the *ſkin*. For as to the epidermis, that is certainly inſenſible, ſeeing you may burn it with hot ſpirit of nitre till you give it a durable yellow taint, without occaſioning the leaſt degree of pain.

The *rete mucoſum* of MALPIGHIUS can hardly be ſeparated from the epidermis in experiments of this kind; wherefore I did not try any upon it, being fully convinced that it has no feeling.

The ſkin is ſenſible, and indeed more ſo than any other part of the body; for in whatever manner you irritate it, the animal makes a noiſe, ſtruggles, and gives all the marks of pain that it is capable of. This great ſenſibility of the ſkin has determined me to take it as a fixed ſtandard of that property; and I reckon as inſenſible, or at leaſt only in a ſmall degree, thoſe parts which may be irritated without diſturbing the animal, while it diſcovers ſigns of pain if the neighbouring ſkin is irritated.

The fat and cellular membrane are not ſenſible of pain, as is very well known, and has been ſhewn by other authors. What is told of DIONYSIUS the tyrant, and is commonly known to hold true in hogs, may be ſufficient to confirm this; *viz.* upon thruſting a needle into them they ſhew no ſigns of pain, till it has got

quite through the fat and reached the flesh below[d].

The *muscular flesh* is sensible of pain, but that is owing more to the nerves than to the flesh itself. For if you tie the nerve of any joint, if there is only one, or the principal trunks, in case there are more, the whole joint is thereby rendered insensible, nor is the animal affected by those applications which hurt the joint when the nerves are tied. For it is very well known that all the muscles are capable of pain, the large hollow ones not excepted, such as the stomach, intestines, and bladder.

But the *tendons* are neither capable of sensation, nor pain. This is the first paradox which I advance against the common opinion, and it has hitherto found but few partizans. Almost all the antient, and some of the most modern authors, as La Faye[e], Heister[f], and Garengeot[g], look upon wounds of the tendons as very dangerous and difficult of cure, and the same opinion has been adopted by Boerhaave, his worthy pupil Van Swieten[h], by Acrell[i] and Quesnay[k].

[d] Commentar. Boerh. tom. III. N° 333. not. [b]

[e] Chirurgie de Dionis derniere edition, pag. 680, 681. [f] Inst. Chir. pag. 423, edit. de 1737. [g] Operat. de chir. tom. III. ch. 7. [h] Tom. I. n. 163. p. 238. [i] Om Friskafor pag. 261. seqq. [k] De la supurat. pag. 222.

I shall

I ſhall ſhew immediately, that this opinion was not altogether unknown before. For Job a Mekren[l], a very experienced ſurgeon, has ſaid that the tendons are very inſenſible, and he quotes as an inſtance that of the *patella*. Bryan Robinson[m] aſſerts, that in a living dog irritations of the tendons did not ſeem to be very painful, but the muſcles were more ſenſible. George Thomson obſerved[n] no commotion from a hurt of a tendon, and Daniel Schlichting[o] was witneſs of the ſame thing both in men and in dogs. But theſe authors are not numerous, and made only few experiments.

For the moſt part I have laid bare the tendon of the *extenſores recti* of the *tibia*, or the *tendo achillis*, after which I have pricked it, cut off part of the fibres, even to one half of them, nay I have cut the tendon acroſs ſo as to leave one half of it intire, which wound Boerhaave looked upon as the moſt dangerous. Theſe experiments I have repeated upwards of a hundred times ſince the year 1746, upon dogs, goats, rats, cats, rabbits, and various other animals, always with the ſame ſucceſs.

The uſe of this experiment is to prove, that if you irritate the muſcular fibres they con-

[l] Obſ. cent. pag. 162. [m] Animal œconomy p. 90. [n] Anatomy of human bones p. 170. [o] Traumatograph. p. 213. Eph. nat. cur. vol. VI. obſ. 24.

tract; but it is not the ſame caſe with the tendon, which you may prick and lacerate as much as you pleaſe, without occaſioning the leaſt motion of the muſcle; as indeed in general, when the muſcle is contracted, there is never any contraction obſerved in the tendon, which WILLIS[p] has taken notice of before, and I have ſeen it a hundred times. Hence therefore it appears, that the tendons have no organ neither of motion nor ſenſation.

The conſtant event of this experiment was, that the animal, whoſe tendon was lacerated, burnt or pricked, remained quiet, without ſhewing any ſign of pain, and when it was let go, provided only a ſmall part of the tendon remained whole, it walked eaſily and without any complaint. I have ſeen a dog walk upon his two hind feet, when the *tendo achillis* of both legs was pierced through the middle, and a kid walk freely, after I had cut away one half of each of the ſame tendons. And in another dog, who had only the tendon of the *ſolearis* muſcle intire, thoſe of the *gaſtrocnemii* being cut through and drawn up into a kind of knot; I obſerved no bad ſymptom as long as I kept him. Wounds of the tendons are likewiſe the moſt eaſily cured of all, only by the help of nature, without any trouble or bad ſymptoms, ſo that there is nothing ſurpriſing in that obſer-

[p] De motu muſcul. p. 118. compare BAGLIVI opera p. 317.

vation

vation of M. la FAYE[q], *viz.* of the tendon of the biceps being cut without occaſioning any want of motion in the arm. Neither ought we to blame VESLINGIUS and ſome others[r] for boldly recommending the ſutor of the tendons; nor M. BIENASE[f] for having ventured upon it, after having firſt tried the experiment on a dog. Dr. ZIMMERMAN[t] obſerved no ſenſation in the *aponeuroſis* of the *abdomen*, upon touching it with oil of vitriol.

After I was fully ſatisfied of the certainty of the above event, I eaſily diſcovered the cauſe, *viz.* that there are nerves diſtributed to the muſcles, but not to the tendons. FABRICIUS[u] AB AQUAPENDENTE owned long ago, that he could not trace the nerves to the tendons, but they diſappeared in a kind of membrane before they got that length; and LEUWENHOEK[x] could diſcover only a very few nervous filaments upon the ſurface of the tendons.

Seeing therefore in the human body the nerves only are capable of ſenſation, it is neither unnatural nor improbable, that the tendons being deſtitute of nerves ſhould have no ſenſation. I have oftener than once ſeen the tendons laid bare in man, and, emboldened by

[q] Chir. de DIONIS pag. 681. note [a]. [r] Epiſtol. poſth. a BARTHOLINO. [f] VERDUC. Operat. de Chirurg. c. 32. [t] Diſſert. cit. p. 16. [u] De Fabric. muſcul. pag. 27. [x] Epiſt. Phyſiolog. pag. 443.

the

the experiments which I made upon brutes, I once laid hold, with a pair of *forceps*, of the naked flexor of the third joint of the fore finger, of a young gentleman, without his being ſenſible in the leaſt that I touched it. I have likewiſe ſeen the tendon of the long *ſupiniator* muſcle chafed with hot oil of turpentine, in order to ſtop an hæmorrhage; it occaſioned an acute pain in the ſkin, but the patient felt none in the tendon. Thus it is an old practice to make uſe of hot oil of turpentine in wounds of the tendons, and ſurgeons have long ago looked upon it as an excellent remedy in ſuch caſes; but this oil would certainly occaſion as much pain in the tendons, as it does in the ſkin, provided they were ſenſible.

Wherefore we need no longer be afraid of wounds of the tendons, of whatever kind they are. If a large tendon is deſtroyed, it is evident the patient muſt halt, or loſe the uſe of a joint, ſeeing the muſcles, being thereby deprived of their inſertions into the bones, can act no longer upon it. But beſides this want of motion, there is nothing elſe to be feared, and ſometimes nature remedies this misfortune in ſuch a manner, by means of a new cellular membrane, and the neighbouring muſcles, that the motion of the joint is performed as eaſily as before. I have ſeen a new cellular ſubſtance of a bluiſh colour, grow in a few days, and unite the ends of the *tendo achillis* that was cut through in a dog.

dog. As ſoon as they were united the animal ſuffered no more inconvenience, and jumped with the ſame agility as before upon the chairs and tables.

Whence therefore can this miſtake ariſe with regard to wounds of the tendons, into which almoſt all the authors have fallen, who have wrote upon the ſubject, even the moſt reſpectable and intelligent of them in other reſpects? It appears to me to be moſt probably owing to the ſignification of the word νεύρου, being confounded with thoſe of τένων and σύνδεσμος, whereby it is made to ſignify both *nerve*, *tendon*, and *ligament*[y]. But a wound in a nerve produces moſt violent ſymptoms, as will immediately appear. Thus I really believe, that in bleeding, a wound of the *median* nerve, and perhaps ſometimes of a branch of the *muſculocutaneous*, which runs down before the median vein, has produced thoſe painful ſymptoms, which are commonly imputed to pricking the tendon of the biceps muſcle, over which the vein runs. PARE' has left us an account of a remarkable inſtance of this in CHARLES IX. of France. It is likewiſe to the great nerves which are diſtributed the whole length of the fingers, that we ought to attribute the cauſes of the unlucky events of ſome kinds of whitlow, the danger of which has long been aſcribed to their ſeat be-

[y] GALEN de uſu part L. XV.

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ing in the ſheaths of the tendons, as was the opinion of GARENGEOT[z] not long ago.

The *ligaments* and *capſulæ* of the articulation, are ſituated in the neighbourhood of the tendons; the former have been deſcribed under the name of *νεύρον*, and the latter are famous on account of the danger alledged to attend wounds of them, and of their being blamed by many celebrated phyſicians as the ſeat of the gout[a].

In making theſe experiments I met with a good deal of difficulty. For as it was neceſſary to remove the ſkin, and to ſtretch it a good deal in the tight joints of the ſmaller kinds of animals, ſo as to allow the wounding and irritating inſtrument to be introduced into the hollow of the articulation, the animal frequently complained, upon my laying hold of the ſkin with the *forceps*. However the experiment, even when it was made with poiſons, very often ſucceeded. In a cat I filled the articulation of the *femur* with the *pelvis* with oil of vitriol, without its ſeeming to feel this violent corroſive, although I have ſeen the uterus of a bitch deſtroyed in the ſpace of a

[z] Opera de Chirurgie. tom. III. pag. 286, 301, 302.

[a] BOERHAAVE *aphoriſ. de cognoſc. & curand. morb.* 1254, 1259. It is true this great man admitts likewiſe the nerves to be the ſeat of this diſeaſe.

minute

minute by the ſame application. In making theſe experiments upon the articulation of the knee, which is more readily come at becauſe it is almoſt bare, I have frequently made uſe of ſmall pieces of ſticks dipt in oil of vitriol, or in the butter of antimony, with which I have burnt the lateral ligaments, that of the *rotula*, both the external and the internal ſurface of the *capſula*, and HAVERS's gland, without the animal diſcovering the leaſt ſenſe of pain. Nay theſe wounds, which are commonly reckoned the worſt, were cured with ſo much eaſe, that the ſaliva alone of the animal was ſufficient for it, and very often they healed without it. Now all theſe experiments, which have been repeated upon dogs, cats, and young kids, juſtify the obſervation of M. DE LA MOTTE [b], who found the *extenſor* ligament of the *tibia* to be inſenſible. Sometimes inſtead of cauſtics, I tried the experiment with a knife or needle thus: I made an inciſion on the external ſide of the articulation of the knee, laid bare the *capſula*, *patella*, the ligament which goes from it to the *tibia*, and the external or internal lateral ligament; then ſcraping with a knife the external ſurface of the *capſula* and ligament, I thruſt a knife or needle thorough from the inſide till its point pierced the ſkin, during all this the animal ſhewed no ſigns of pain, till the point of the knife or needle, after having pierced the

[b] Chirurg. compl. N° 365.

capſula

capſula of the anticulation, came to touch the cellular membrane under the ſkin. This experiment I have repeated ſeveral times.

Hence it appears, that we ought not to attribute the ſharp pains of the gout to the *capſula* of the joint, in which it is ſo difficult to find any nerves, and which is void of ſenſibility: Their true ſeat is in the ſkin, and in the nerves which creep upon its internal ſurface. And it was certainly very well ordered by nature, that thoſe parts which are expoſed to a continual friction ſhould be void of ſenſation.

Wherefore if wounds of the joints are ſometimes difficult to heal, we muſt attribute this to the humour which is continually ſecreted there, and which readily acquiring a rancid putrefaction, will not allow the wound to heal up. In dogs, as far as I can remember, theſe wounds always healed without any difficulty.

The *perioſteum* reſembles the ligaments and *capſulæ*, and evidently in the *fœtus* forms together with them a thick pulpy membrane, which is continued from one bone to another, including the intermediate articulation. Wherefore I was not ſurpriſed to find it inſenſible in the numerous experiments which I made in the leg, thigh, heel, *metatarſus*, and *pericranium*, which laſt is of the ſame nature with the *perioſteum*.

Phyſicians,

Phyſicians, anatomiſts[c], and ſurgeons, who are of another opinion, will I hope pardon me for differing ſo widely from them, and defer condemning me, till they have compared the experiments upon which that opinion is founded. A hundred times have I tore, pricked, and burnt the perioſteum, without the animal's ſhewing the leaſt ſenſe of pain; nay young kids have ſucked during the time, while if I pinched the ſkin they complained violently, and fell into convulſions.

This inſenſibility of the perioſteum was obſerved before by Mr. Cheselden, and it is not at all ſurpriſing in a part where there are no nerves, where even Nesbit[d] himſelf could not diſcover any, but concluded there muſt be ſuch, though they were inviſible, to account for the ſenſibility which he aſcribed to this membrane. For thoſe which creep in great abundance upon the *pericranium*, and do not come from the tenth pair, but from the ſecond and third of the neck, and from the third and fifth of the brain, are beſtowed upon the ſkin of the head, and communicate their ſenſibility to it.

[c] Winslow, Traité des os frais N° 60. Clopton, Havers, Nesbit *human. oſteogon.* p. 6. Phil. Ad. Boehmer. oſteol. p. 31. Duverney Traité des maladies des os, tom. II. p. 431. [d] Loc. cit. ut ſupra.

The inſenſibility of the *bones* is ſtill diſputed, and indeed I have made no experiments upon this ſubject, for in that cruel torture which is unavoidable in order to lay bare the bones, it is very difficult to diſtinguiſh the new pains thereby produced. The ſenſibility of the *teeth* is known to every body, but the ſame reaſon which explains that, perſuades me that the bones are void of ſenſation. For though the nerves of the teeth are extremely ſlender, yet they are eaſily ſhewn where they enter their proper foramen. But in the large bones I have never been able to find any nerve, accompanying the artery and vein at their entry into the bone[e]; if there were, I ſhould have diſcovered them in my numerous deſcriptions of the arteries; if not elſewhere, at leaſt in the large and ſmooth internal ſurface of the cranium, and in my preparations of the nouriſhing arteries of the whole body. DIDIER[f] ſays that the bones rendered ſoft are very ſenſible; but beſides that it is eaſy to be impoſed upon in ſo terrible a diſeaſe, M. IMBERT[g] aſſerts the contrary; and I have ſeen the operation of the trepan performed upon ſound perſons who had the free uſe of their ſenſes, without their complaining of any pain from the perforation of the *cranium*.

[e] Nervi ad oſſa nulli, RIOLAN enchirid. Anatomic. AL. MONRO loc. cit. p. 16.

[f] Anat. raiſ. p. 6, 7.

[g] Quæſt. med. XII. p. 33.

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DEVENTER[h], AMBROSE PARE[i], DU VERNEY[k], and moſt other authors, agree in ſaying that the *marrow* is very ſenſible of pain; but this is very improbable, as it is of the ſame nature with fat, and has no nerves beſtowed upon it.

The *dura mater* is a kind of *perioſteum*, every where applied to the internal ſurface of the *cranium*, connected to it by veſſels, and has arteries tranſmitted through it to the ſkull, in the ſame manner as they are tranſmitted through the perioſteum to the other bones of the body. PACCHIONI and BAGLIVI have aſcribed to it a force equal to that of the heart, and the moſt part of phyſicians have looked upon it as the ſeat of the moſt violent diſeaſes; but their opinions do not change the eternal nature of things. I have ſhewn elſewhere[l], that it was, like all the other membranes of the body, compoſed of the *membrana cellularis*, and this analogy has been confirmed by the experiments of Meſſieurs ZINN[m], ZIMMERMAN[n], and CASTEL, and likewiſe by my own, which convince me that this membrane, ſo nearly reſembling all the others produced from it, may be burnt with oil of vitriol, butter of antimony, or ſpirit of

[h] Van beenſiekten p. 80. [i] Adminiſtr. anat. p. 83. [k] Mem. de l'acad. des ſcienc. 1700. pag. 205. [l] Prim. lin. phyſiolog. n. XI. [m] Experim. circa corpus callo-ſum cerebellum &c. Gotting. 1749. p. 28. ſeqq. [n] Loc. cit. p. 6. &c.

nitre, cut with a knife, or tore with a pair of pincers, without the animals appearing to ſuffer the leaſt pain. Meſſieurs ZINN and MEKEL obſerved the ſame inſenſibility in the *dura mater* of a man, which had been laid bare by a *caries* of the *cranium*; and doubtleſs the antient phyſicians, CARDANUS[o], and before him GALEN, ſpoke from experience, when they recommended the ſharpeſt applications as not only ſafe, but requiſite in treating the *dura mater*. Comparative anatomy likewiſe informs us, that this membrane is a true cover for the brain, and not a muſcle; for in the tortoiſe it is found of a cartilaginous conſiſtence[p].

How is it poſſible, that a membrane ſo inſenſible and incapable of motion, ſhould have the force to ſend back the ſpirits to the heart, and be the ſeat of head achs, the *phrenitis* or madneſs, unleſs you chuſe to alledge, that when it is affected, the brain by its proximity ought to ſympathiſe? wherefore the French ſurgeons are very right in venturing to cut it, as often as they obſerve *pus* or extravaſated blood under it.

I muſt here beg leave to make a digreſſion, which perhaps may be of uſe. M. SCHLICHTING[q] has ſaid, that there was a motion in the brain, that it aſcended and deſcended al-

[o] De vulner. cap. p. 139. [p] STEPH. LORENZINI obſ.
[q] Memoir. préſentes tom I. p. 114. *et ſeq.*

ternately,

ternately, and is extremely angry with thoſe who rank it amongſt thoſe parts of the body which are void of motion. Being fully convinced of the ſtrong adheſion of the *dura mater* to the *cranium*, and of the total plenitude of the contents of the ſkull, I could not help wondering at the boldneſs with which this author maintained the contrary; however I did not think of refuting him by authorities or reaſons *a priori*, but judged it beſt to fight him with his own weapons, that is by experiments. Wherefore I opened the ſkulls of ſeveral dogs with a hammer and chiſſel, which is a more commodious way than with a trepan, and expoſed to view a large part of the brain; and this experiment I frequently repeated upon dogs, goats, rats, frogs, cats, and other animals. The event was always the ſame, *viz.* I ſaw a manifeſt motion in the *dura mater*, or rather in the whole brain, ſuch as SCHLICHTING has deſcribed, for it aſcended in expiration and deſcended in inſpiration. Upon account of this motion alone I made above thirty experiments with M. WALSDORF, who propoſes ſhortly to publiſh a ſmall treatiſe upon this ſubject [r].

I am too great a lover of truth to ſuffer pain upon account of a new diſcovery, however oppoſite it may be to my former ideas; but what gave me uneaſineſs was, that I could not diſ-

[r] He publiſhed it in 1753, ſince the firſt publication of this treatiſe.

cover the reaſon of this correſpondence between the motion of the brain and reſpiration: For the mind loſes patience at the diſcovery of a phænomenon which ſeems repugnant to reaſon.

But repeated experiments removed this ſeeming contradiction. Neither the dura mater nor the brain have any motion unleſs the cranium is removed, which therefore in a living ſound animal is an effectual obſtacle to it. This SCHLICHTING himſelf owns to be the caſe, nay for the moſt part there appears no motion in the brain, till the adheſion of the dura mater to the bones of the *cranium*, which keeps the brain immoveable, is deſtroyed by the fingers or ſome proper inſtrument. Wherefore this conſent of the motion of the brain with reſpiration can never be applied to a ſound healthy perſon; for if the dura mater is not moved ſo long as it adheres to the cranium, and if the brain is only raiſed up in expiration after the dura mater has been ſeparated from the ſkull, which is never the caſe of a perſon in health, this phænomenon ought not to be regarded as really exiſting.

Farther, I did not find this peculiar to the brain, but from repeated experiments I have obſerved the ſame in the larger veins, *viz.* in both the *cavæ*, the ſubclavians, the upper part of the baſilic, and the jugulars; for all theſe ſwell during

[s] Ibid. pag. 516.

expira-

expiration, and become of a deep blue colour, whereas in inſpiration, being more empty, they ſubſide and look pale. The phænomenon therefore which SCHLICHTING has obſerved is not peculiar to the brain, as I took notice of before, and ſeems intirely to depend upon that facility with which the blood paſſes from the right ventricle of the heart into the lungs, which are dilated while we inſpire, and upon that with which the *venæ cavæ* empty themſelves into the ſame ventricle in the time of inſpiration[t]. On the contrary, in exſpiration the lungs being compreſſed, reſiſt the paſſage of the blood from the heart, and the blood contained in the heart reſiſts that returning from the extremities; hence the large veins, and amongſt theſe the jugulars ſwell, and thereby the brain is rendered turgid with blood[u]. I know very well that by prolonging inſpiration voluntarily, we retard the blood which paſſes through the lungs[x]; but in the ordinary alternate motion of reſpiration, the blood enters more eaſily into the lungs during inſpiration, although in a preternatural ſtate, when the lungs are filled with blood, and for want of expiration, it cannot paſs to the left ventricle, there muſt thereby be produced a dilatation of the right, and a ſtagnation in the veins, almoſt equal to that which naturally accompanies expiration.

[t] Primæ Lineæ Phyſiol. N° 292. [u] Ibid. § 297.
[x] Ibid § 294.

Allow me to add in two words, that the longitudinal ſinus has no pulſation, even after the *cranium* has been removed, and that when it is pierced, the blood does not fly out in jirks, but runs in a continued ſtream, as when one opens a vein; and this confirms what I publiſhed before againſt the pulſation of the ſinuſes of the brain[x]. It is owing to the ſame cauſe, that the ſmall arteries which go from the *dura mater* to the *cranium*, and of which the greateſt number take their origin from the ſurface of the large *ſinus*, may be filled with injection, without its ever penetrating into the ſinus itſelf.

The followers of STAHL, and others, eſpecially GOHL, who deny the exiſtence of the animal ſpirits, conceive the nerves to be like tenſe cords, which are put in motion by the impreſſions of objects, and communicate their vibrations to the *meninges* of the brain, which they look upon as the organ of all our ſenſations. This theory I have refuted by ſeveral arguments, which I ſee have not only ſatisfied Dr. FLEMMING[y], but likewiſe the moſt modern patrons of the doctrine of the ſoul governing the body, admit the animal ſpirits, as Dr. WHYTT.

But there is ſtill another argument, which proves more fully, that the faculty of ſenſati-

[x] Comment. ad inſt. BOËRH. N° 234. [y] Of the nature of the nervous fluid, Lond. 1751. 8vo.

on,

on, whatever it is, does not reſide in the membranes of the nerves. With regard to the *dura mater* I am fully convinced, that it does not form the external coat of the nerves, although moſt other anatomiſts are of a different opinion. But there remains the *pia mater*, which actually ſheaths each of the medullary fibres, which are ſo very ſlender, that we can count near a hundred of them in one of the branches of the fifth pair. If therefore I can ſhew that the *pia mater* is void of ſenſation, there can be no reaſon given, why the ſenſibility of the nerves, which is owing to the *medulla*, ſhould be aſcribed to the membranes. In order to this, I have tried the following experiment upon dogs and kids, and frequently repeated it.

I laid bare the *pia mater*, by removing a part of the ſkull and the *dura mater* contiguous to it; I touched it with butter of antimony (which ought in this caſe to be preferred to the oil of vitriol, as this conſumes the membranes too faſt, and it is almoſt impoſſible to prick the *pia mater* with a knife without wounding the brain at the ſame time) whereupon it was burnt to an *eſcar*, without the animal's complaining in the leaſt, making any ſort of ſtrugling, or being at all convulſed. But upon touching the brain, in whatever manner I did it, the animal was inſtantly ſeized with violent convulſions, which bended its body to one ſide in the form of a bow.

If the *dura* and *pia mater*, and likewiſe the *periofteum*, are found void of ſenſation, it appears evidently, that the ſenſibility of the *other membranes* muſt be given up. This conjecture is confirmed by repeated experiments, which I have made very carefully upon the *peritonæum*, after removing the *recti* muſcles, upon the *pleura* freed from the intercoſtal muſcles and nerves (which is a difficult experiment, but I have frequently done it, and moſt ſucceſsfully on a kid, it being a very quiet animal) and even upon the *pericardium*, in all which caſes the membranes being cut or irritated, occaſioned no ſenſe of pain, nor the leaſt change in the animal. The celebrated STORCH, as appears by the journal of the diſeaſe of which he died, was not ſenſible of any pain, while, in performing the *paracenteſis*, the *trocar* pierced the *peritonæum*. Upon this occaſion I foreſee that many eminent phyſicians, who place the ſeat of the violent pain of the pleuriſy in the membrane of the *pleura*, will differ from me in opinion. But I only relate facts which I have actually ſeen.

Neither ought theſe truths to be looked upon as paradoxes, becauſe they contradict ſome pathological doctrines. BOERHAAVE[z] affirmed long ago, that in inſpiration the *pleura* was leſs upon the ſtretch, becauſe by the ribs at that time approaching nearer to one another,

[z] In his *Praxis Medica*, 1745, tom IV. pag. 162.

the intervals between them were thereby diminiſhed; and that on the contrary, in expiration, that membrane was more tenſe, becauſe then the ribs receded to a greater diſtance from each other. But in the pleuriſy the patient ſuffers moſt in inſpiration, that is, when the *pleura* is leaſt diſtended, and *vice verſa*; wherefore that great man did not place the ſeat of this diſeaſe intirely in the *pleura*, but he likewiſe joined to it an inflammation in the muſcles which ſerve to bring the ribs nearer to one another. But according to my ſyſtem, in order to explain this phænomenon, it is ſufficient that the intercoſtal nerves ſuffer, be the caſe as it will in other reſpects.

The *mediaſtinum*, being a very fine membrane, and very much reſembling the *omentum*, I conſider in the ſame light with the *pleura*. All theſe membranes being deſtitute of nerves, are of the ſame nature with the cellular membrane, and therefore are not ſenſible of pain.

The *arteries* and *veins* ſeem not ſuſceptible of pain; for upon irritating or laying hold of a nerve the animal complains, but if you lay hold of an artery it does not feel it. The ſenſibility which is occaſionally met with in the membranes of the carotid, the lingual, temporal, pharyngal, labial, and thyroidal arteries, and of the *aorta* near the heart, depends upon the nerves which I commonly demonſtrate there,

and

and which do not appear to make any farther progreſs. In thoſe places it is natural to imagine, that the arteries muſt be ſenſible, ſeeing the nerves run immediately upon them, but otherwiſe they are endowed with little or no ſenſation. I have frequently ordered the arteries to be tied very tight in men, without their complaining of pain in the leaſt.

The *internal membranes* of the *ſtomach*, *inteſtines*, *bladder*, *ureters*, *vagina* and *womb*, being of the ſame nature with the ſkin, ought of conſequence to enjoy the ſame ſenſibility.

The *heart* is likewiſe ſenſible, which I don't ſay from my own experiments, but from thoſe of other perſons; and it is natural to think it ſhould, ſeeing it is a muſcle, and has nerves beſtowed upon it. The reaſon why I have not diſcovered it myſelf is, that an animal whoſe *thorax* is opened is in ſuch violent torture, that it is hard to diſtinguiſh the effect of an additional ſlight irritation.

On the other hand, I am fully convinced, from a great number of experiments, that the *viſcera*, properly ſo called, *viz.* the *lungs*, *liver*, *ſpleen* and *kidneys*, have very little ſenſation; ſeeing I have irritated them, thruſt a knife into them, and cut them to pieces, without the animal's ſeeming to feel any pain. Dr. ZIMMERMAN

MERMAN[a] has tried the ſame experiments with the like ſucceſs. Hence ulcers of the lungs are not attended with any pain, and perſons have had ſtones in their kidneys for ſeveral years without ever being ſenſible of them.

If it ſhould be objected that there are nerves in theſe viſcera, my anſwer is, I do not alledge that they are void of all ſenſation, but only that it is very weak in them, *viz.* ſuch as one would expect in a part which has very few nerves beſtowed on it in proportion to its bulk. For all the *viſcera* have large blood veſſels and ſmall nerves, the liver not excepted, but eſpecially the ſpleen and the kidneys.

The *glands* in general have only an obtuſe ſenſation, owing to the few nerves which are beſtowed upon them. Hence ſchirrous and encyſted tumours are ſo indolent, and it is very ſurpriſing, that M. DU BORDIEU, ſo ſevere a critic of the writings of others, ſhould lately have eſtabliſhed it as an axiom, that a great many nerves are beſtowed upon the glands, and upon this founded a ſyſtem to explain the nature of their functions, in which he alledges, that it is not compreſſion, but irritation that makes them diſcharge their liquors. In the mean while it is eaſy to prove, that there are no conſiderable nerves beſtow'd upon the largeſt of the glands nor the *thymus*, as far as hitherto we have been able to diſcover; that

[a] Loc. cit. p. 17.

the nerves which go to the thyroid gland are a great deal ſmaller than thoſe of a muſcle ten times leſs in bulk than that gland, and that there is not a gland in the whole body, in which we can demonſtrate any nerve of the larger ſort. Beſides upon opening the mouth, when we are not at all hungry, we can ſee the *ſaliva* ſqueezed out, by the ſole compreſſion of the *digaſtric* muſcle.

The breaſts are covered with a great deal of ſkin, and furniſhed with many nerves. The *pænis*, which has likewiſe a great deal of ſkin, and receives a greater number of nerves than any other part of the body of an equal ſize, has alſo a proportionable degree of ſenſibility. The *tongue*, which has likewiſe a great many nerves, is endowed with a ſenſation more acute and delicate than the touch, which form the taſte. We may judge of the ſenſibility of the *eye*, and eſpecially of the *retina*, from the irritation and inflammation occaſioned by too glaring a light. The *tunica choroidis* and the *iris* ſeem likewiſe to be ſenſible. But I do not find that the *cornea* has any nerves, and hence it may be pierced with a needle without occaſioning any pain; and what perſuades me that the *iris* is much leſs ſenſible than the *retina*, is, that if after having pierced the *cornea*, you irritate or cut the *iris*, it is not thereby contracted, whereas the leaſt increaſe of light makes it contract; which evidently proves, that this contraction does not depend upon the

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proper ſenſibility of the *iris*, but on that of the *retina*. The *gutta ſerena* ſerves likewiſe to prove the ſame thing; the *iris* being no ways changed in that diſeaſe, any farther than that it is deprived of motion, from the ſenſation of the *retina* being deſtroyed by a palſy of the optic nerve.

The *nerves*, which are the ſource of all ſenſibility, are themſelves of courſe extremely ſenſible. It is impoſſible to repreſent to one's ſelf, without having ſeen it, the vaſt pain and diſquietude which an animal is thrown into, upon touching, irritating, or even tying a nerve. I have learnt from experience, that by tying a conſiderable branch, not only of the eighth pair, but even of the extremities, dogs have died after a few days; which has made me ſtill more afraid than I was before of tying the nerves ſo common in amputations. But if a nerve is cut, and irritated below the ſection, the animal feels no ſenſation therefrom, which is a proof that pain is not propagated from one nerve to another by their anaſtomoſing.

We have ſeen that the ſenſible parts of the body are the *nerves* themſelves, and thoſe to which they are diſtributed in the greateſt abundance; for by intercepting the communication between a part and its nerve, either by compreſſion, tying, or cutting, it is thereby immediately deprived of ſenſation, which is a fact

proved

proved by well known experiments, ſome of which you may ſee in my commentaries upon BOERHAAVE [b]. Wherefore the nerves alone are ſenſible of themſelves, and their whole ſenſibility reſides in their medullary part, which is a production of the internal ſubſtance of the brain, to which the *pia mater* furniſhes a coat.

SECTION II.

I proceed now to irritability, which is ſo different from ſenſibility, that the moſt irritable parts are not at all ſenſible, and *vice verſa*, the moſt ſenſible are not irritable. I ſhall prove both theſe propoſitions by facts, and at the ſame time I ſhall demonſtrate, that irritability does not depend upon the nerves, but on the original fabric of the parts which are ſuſceptible of it.

In the firſt place the nerves, which are the organs of all the ſenſations, are not at all irritable. This will appear ſurprizing, but at the ſame time it is very true. If you irritate a nerve, the muſcle to which it is diſtributed is immediately convulſed. I never ſaw this experiment fail, and I have frequently by this means ſeen the diaphragm and abdominal muſcles of a rat, and the hind and fore legs of a frog, thrown into convulſions. You may ſee experiments of the ſame nature in SWAM-

[b] De irritabilitate n. 284. not. g.

MERDAN,

MERDAM, and in making them I have found, as OEDER[c] mentions, that the irritation of a nerve communicates the motion only to those muscles to which it is distributed, and does not effect those which derive their nerves elsewhere.

I have likewise constantly remarked, that the convulsion of a muscle only took place when the nerve was irritated with a knife, and not when the experiment was made with corrosives.

But if you irritate the nervous fibres which are scattered through a muscle, there is thereby no contraction produced in the nerve. I have often carefully observed in dogs, and more especially in frogs, that whatever irritation I gave to a muscle, it never communicated the least motion to the nerve.

I afterwards tried the same experiment which ZINN made at Berlin, viz. I applied a mathematical instrument, marked with very small divisions, lengthways to a long nerve of a living dog, in such a manner as I could thereby perceive the slightest contractions; and upon irritating the nerve, it remained perfectly quiet.

These experiments clearly prove, by the by, that the force of oscillation, which has been attributed to the nerves, is not agreeable to experience.

Neither the skin, which is the seat of feel-

[c] De irritabilitate n. 22. p. 5.

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ing, nor the nervous membranes of the ſtomach, inteſtines, and *Urethra*, are irritable. Here we muſt take care not to confound with this property, a kind of vermicular motions produced by the corroſion which oil of vitriol, or ſpirit of nitre, communicates to the nerves, or arteries, when they are cut into pieces, or the contraction of the ſkin of the membrane of the *Urethra*, of the bladder, or gaul bladder, occaſioned by the ſame application. The lungs have been conſtricted by oil of vitriol after death according to ZIMMERMAN [d]. And a motion has been obſerved ſome hours after death, in the ſkin, fat, and tails of animals [e]. Wherefore this corroſive force has nothing in common with life, and continues to take effect twenty four hours after death, as I have learnt by experience.

Neither is irritability proportioned to ſenſibility. The ſtomach is extremely ſenſible, and the inteſtines are rather leſs ſo, ſeeing they certainly are not liable to ſuch violent pain, and yet I have found them to be more irritable. The heart, which is very irritable, has but a ſmall ſhare of ſenſation, and upon touching it in a living perſon, a fainting is thereby occaſioned rather than pain.

Farther, we muſt not conclude that a part is ſenſible, becauſe it is irritable; for the tying or cutting of a nerve, which deſtroys the ſenſibility of that part to which it is ſent,

[d] De irritabilitate p. 17. [e] *Ibid.* p. 13.

does

does by no means deſtroy its irritability. I have frequently repeated BELLINI's experiment, but the event was ſomewhat different from what is commonly related, viz. I laid hold of, and compreſſed the phrenic nerve of a living animal, or of one very lately dead, for the experiment ſucceeds alike in both caſes; this compreſſion irritating the nerve, put the diaphragm in motion; if I tied the nerve the ſame thing happened; if I cut it, and irritated it below the ſection, where there was no ſenſation left, becauſe the communication with the brain was thereby deſtroyed, the diaphragm was convulſed the ſame as before. In the ſame manner by cutting the crural nerve of a dog, you thereby deprive the limb of all ſenſation, and you may tear it as much as you pleaſe without putting the animal to pain; but at the ſame time if you irritate the nerve which has been cut, the muſcles of the leg are ſeized with a trembling motion; wherefore at that time it is irritable, though quite inſenſible. This experiment of BELLINI has been too much embelliſhed. It is true that the compreſſion and irritation of the nerve puts the diaphragm in motion, but this is equally the caſe whether you preſs the nerve from above downwards, or from below upwards. The experiment ſucceeds better if the nerve is tenſe than if it is relaxed. If you compreſs the nerve, and irritate it above the compreſſed part, in whatever manner you do it, the dia-

phragm remains without any motion, and therefore it is not true what FREDERICK ORTLOB[c] has ſaid, viz. that it is put in motion if you direct the compreſſion of the nerve downwards, and ceaſes upon ſliding the finger towards the upper part of the *thorax*.

Laſtly, in ſmall animals I have tied the trunk of the nerves which go to the extremities, and thereby rendered the limbs inſenſible and paralytic. Afterwards I have irritated the muſcles, and ſeen them contract the ſame as before, though they were no longer ſubject to the command of the will.

I have tried experiments of the ſame kind upon parts ſeparated from the body. The inteſtines in this ſtate after being deprived of all communication with the brain, preſerve their periſtaltic motion; and if you touch them with a knife or corroſives, they put on the ſame appearances, as if they were in their natural ſituation, and ſtill preſerved their connexion with the nerves and brain[f]. The ſame thing is to be obſerved in the heart, and in any other muſcle cut off from the body[g]. In an eel, the heart continues its motions during the ſpace of ſeveral hours, with the greateſt regularity, even after it has been pulled out of the *thorax*.

If therefore we ſay that an animal only feels,

[c] In præf. ad *anatomen. rationalem* DANIELIS TAURY. [f] WOODWARD's ſupplement, pag. 76. [g] ZIMMERMAN, pag. 19.

when any external impreſſion is repreſented to the mind, certainly that part of the body muſt be void of ſenſation, whoſe communication with the brain is deſtroyed by the nerve being cut, or the part being taken quite out of the body. In aſſerting that there was no motion of our body but by the ſoul, Dr. WHYTT has found himſelf obliged to admit the diviſibility of the ſoul, which he believes to be ſeparable into as many parts as the body[h]. I have frequently repeated the experiment juſt now mentioned: *viz.* I pulled out the inteſtines as quickly as I could, and cut them into four or eight pieces, all which moved ſeparately, ſtill preſerved their periſtaltic motion, and contracted in whatever manner they were irritated. Dr. WOODWARD[i] tried experiments of the ſame kind before in the inteſtines, and BAGLIVI[k] upon the heart of a frog, as did AURELIUS SEVERINUS[l] long before the others. I have ſeen the heart divided into ſeveral ſmall pieces, each of which moved upon the table. M. LUPS[m] has obſerved in the membranes of an egg an irritability which cannot proceed from any nerve, becauſe there is no ſuch thing there; but I have had no experience upon this article. I find that BAGLIVI[n] has made uſe of the ſame arguments to eſtabliſh the ſeat of irritability in the ſolids, and we ought to take care not to

[h] L. C. pag. 383. [i] L. C. pag. 80. [k] De fibra motrice pag. 7. [l] Vipera pythia, pag. 119. [m] L. C. pag. 34. [n] De fibra motrice & morboſa, p. 7.

call in here the analogy of insects, which are both irritable and sensible all over *.

The soul is a being which is conscious of itself, represents to itself the body to which it belongs, and by means of that body the whole universe. I am myself, and not another, because that which is called I, is changed by every thing that happens to my body and the parts belonging to it. If there is a muscle, or an intestine, whose suffering makes impressions upon another soul, and not upon mine, the soul of that muscle or intestine is not mine, it does not belong to me. But a finger cut off from my hand, or a bit of flesh from my leg, has no connexion with me, I am not sensible of any of its changes, they can neither communicate to me idea nor sensation; wherefore it is not inhabited by my soul nor by any part of it; if it was, I should certainly be sensible of its changes. I am therefore not at all in that part that is cut off, it is intirely separated both from my soul, which remains as entire as ever, and from those of all other men. The amputation of it has not occasioned the least harm to my will, which remains quite entire, and my soul has lost nothing at all of its force, but it has no more command over that amputated part, which in the mean while continues still to be irritable. Irritability therefore is independent of the soul and the will.

These same experiments farther prove, that

* Theolog. des insect t. 2. pag. 84, 85.

the

the whole force of the muſcles does not depend upon the nerves, becauſe after theſe have been tied or cut, the muſcular fibres are ſtill capable of irritability and contraction; and ſome time or another perhaps, the uſe of the nerves with regard to the muſcles will be reduced to convey to them the commands of the ſoul, and to increaſe and excite that natural tendency which the fibres have of themſelves to contract, in whatever manner that is brought about.

But I return to my experiments, whereby I have diſcovered which are the irritable parts of the human body, and in what degree they are ſo.

I have excluded the ſkin. The cellular membrane and the fat, which the oil of vitriol devours ſo greedily, is generally agreed to be void of motion, at leaſt a gentle irritation does not affect it. So neither the lungs (tho' violent acids conſtrict them) the liver, the kidneys, nor the ſpleen, have any irritability; becauſe they are compoſed of the cellular ſubſtance, which, as I ſaid juſt now, is not endowed with it, and of veſſels which are equally void of irritability.

This character of irritability ſeems to me to be that which diſtinguiſhes the cellular fibres from the muſcular, which reſemble each other ſo much, that they are confounded together every day, as appears for example in the *dartos*, which ſo many ſtill look upon as a muſcular

membrane, as alſo in GLISSON's *capſula*, and the round ligament of the womb, in which a great number of anatomiſts have been at a vaſt deal of pains to diſcover muſcular fibres.

The diſpoſition of the cellular membrane to irritability, is preciſely the ſame with that of the fibres of dead fleſh; it yields to the touch, dimples if you preſs it, and recovers itſelf if the preſſure is removed. If you cut it, the fibres retract on both ſides, and leave an empty ſpace between. But the muſcular fibres, if you irritate them in a living body with a knife or corroſives, become ſhorter; their extremities approach nearer to one another, preſently they are relaxed again, and theſe contractions and relaxations are repeated alternately for ſome time,

The *tendons* are as void of irritability as they are of ſenſation. No irritation made with a knife or with any gentle corroſive can excite any convulſion in them, or produce any motion in the muſcle to which the irritated tendon belongs. If the electric ſpark produced from the *tendon* is violent, the celebrated M. JALLABERT has obſerved, that ſparks of the ſame kind may be produced equally as violent from other parts of the body which are more ſolid and hard.

The ligaments, *perioſteum*, *meninges* of the brain, and all the membranes, being compoſed of the cellular membrane, are void of irritability: and the following experiment may ſerve to convince thoſe of their error, who have fancied that they

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have obſerved muſcular fibres in the *dura mater* and *pericardium*; *viz.* if you cut, burn, or tear thoſe membranes, you can obſerve no ſenſible motion excited in them thereby. I have repeated this experiment an hundred times, and the ſame has likewiſe been done by Meſſ. Zinn, Walstorf, Castel, Oeder and others, always with the ſame ſucceſs.

The *arteries* by ſome are thought to be irritable, both upon account of their muſcular membrane, and the neceſſity of finding the cauſe of their contraction, which is conſtantly alternate to that of the heart; and we know that Drs. Senac and Whytt° have looked upon this irritability as eſſential to the arteries. The firſt of theſe authors takes it for a more efficacious cauſe of the circulation, than the heart itſelf; and I own that this ſyſtem has an appearance of reality. The inteſtines, which by their periſtaltic motion puſh on their contents; the principal artery of the ſilk worm, which performs the office of a heart; and animals in whom after the heart has been cut out, the motion of the fluids has been continued for ſome time, which appears to be owing entirely to the arteries; laſtly, local inflammations occaſioned by irritating topics, form ſo many analogies, which unite to render this ſyſtem probable. Upon examining with the microſcope the blood in a fiſh, and in a frog, after the heart was pulled out, it continued for ſome time to move in the veſſels,

° L. C. p. 95.

and

and I ſaw it paſs up and down the veſſels of a ſmall fiſh, which had no motion neither in the heart nor gills, nor ſhewed the leaſt ſign of ſenſibility.

In the mean while all thoſe facts do not yet prove the irritability of the arteries. Irritate the *aorta* of any animal you pleaſe, either in its internal or external ſurface, and either with a ſharp inſtrument, corroſives, or hot ſpirit of nitre, you will not be able to obſerve any contraction thereby produced, except that from oil of vitriol, which I mentioned before[p], and which ſucceeds equally as well, if the experiment is made ſeveral hours after the animal has been quite dead. In living frogs I have frequently irritated the arteries with alcohol, ſpirit of nitre, and other acrid liquors; and though I examined them attentively during the time with a microſcope, I could never diſcover any motion thereby occaſioned in them, while the blood which they contained was changed into a thick jelly of the colour of earth.

Farther, upon examining the circulation in animals with the microſcope, I have never obſerved any contraction in the arteries. I have viewed the circulation for whole hours in fiſhes and frogs, and during the whole time the ſides of the veſſels remained as quiet as thoſe of the tube with which I examined them. If the pulſe of the artery had occaſioned any motion in the neighbouring vein, I could not have failed to

[p] I. G. Zimmeraan p. 24.

diſcover it with the microſcope. With regard to the experiment which HEYDE[q] relates, *viz.* that upon cutting an artery of a frog, it contracts itſelf ſo as to become impervious, I have frequently ſeen the event very different, the orifice of the artery retaining its former figure, without being either contracted or dilated.

Wherefore, although I will not abſolutely deny the irritability of the arteries, yet I do not at all ſee that it is confirmed by experiments.

Neither am I more convinced that it obtains in the *veins*. It is true I can perceive a motion in them, which depends upon reſpiration, and I have frequently obſerved, eſpecially in frigid animals, that of the *vena cava*, which contracts near the heart, and drives the blood which it contains into the auricle. We likewiſe know that upon touching the veins with ſome very ſharp corroſive, as ſpirit of vitriol, or hot ſpirit of nitre, they contract much more ſenſibly than the arteries, and puſh on the blood, as I have ſeen both in a kid, and alſo in a cat. But as neither the knife, nor gentle corroſives injected, produce ſuch a contraction, and there is no acrimony of that force in any of the humours of the human body, I look upon the veins as intirely void of irritability, or at leaſt very gently endowed with it.

The *lacteal veſſels* contract and empty themſelves, upon being touched with oil of

[q] Obſ. 35.

vitriol,

vitriol, and what proves that they have a conſiderable ſhare of irritability is, that though they are ever ſo full of chyle at the time when the animal dies, they empty themſelves, and contract in ſuch a manner, that you cannot diſcover any cavity remaining.

The different *excretory ducts* are not endowed with more irritability than the veins. The gall-bladder, the *ductus coledochus*[r], the ureters, and the *urethra* contract themſelves upon being touched with a very acrid corroſive, but the application of a weak one, or the knife, produces no change in them.

The ureters cannot be irritated even by oil of vitriol, and therefore they ſeem to be deſtitute of any muſcular force, as indeed there has never been any muſcular fibres plainly ſhewn in thoſe canals.

I have convinced myſelf by an experiment of the nature of the bladder. For by pricking it with a knife or needle, in a dog that was almoſt dead, I have ſeen it, though not always, very frequently contract itſelf conſiderably, and drive out the urine through the opening of the abdomen. I have likewiſe obſerved it immediately after death contract itſelf ſpontaneouſly, and empty itſelf of all the urine with which it was full before; which obſervations were formerly made by WEPFER[f], and I have quoted them before from him.

[r] I. G. ZIMMERMAN 46 ab oleo vitrioli.

[f] De cicuta aquat. p. 250.

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The watering of the eyes, which irritating *effluvia* or ſnuffs produce, and the diſcharge of *mucus* from the *urethra*, occaſioned by ſharp injections, together with other *phænomena* of the ſame kind, prove that the glands and mucous ſinuſes in man are irritable; but in the experiments which I have tried upon living animals, I have not diſcovered any ſuch property in theſe parts.

The *uterus* in quadrupeds is irritable, and at leaſt as ſenſible of motion as the inteſtines, whether it ſtill remains in the body, or has been cut out of it. The ſtrong contraction of the human *matrix*, which in a great meaſure expels the child, and is ſo evidently felt by the hand when it is introduced into it, proves its irritability, and this determined RUYSCH to leave the delivery of the after-birth to nature, as every body knows.

The Irritability of the genitals ſeems to be of a particular nature, in ſo far as voluptuous ideas are the moſt proper *ſtimulus* to put them in motion. However ſo far it reſembles that of the other parts, as it is rendered active and produces an erection, if it is excited by a quantity of urine, or *ſemen*, the acrimony of cantharides, or the ſharp diſcharge of a *gonnorrhœa*. But in whatever manner theſe irritations are produced, the effect of them is always to conſtrict the veins, and to retard the motion of the blood through them. As Dr. WHYTT aſcribes the erection of the *penis* to a greater afflux of arterial blood, he

ſeems

ſeems to be unacquainted with the experiments, whereby it appears, that by tying the *penis* either in men or brutes, an erection is produced, and that in the *paraphimoſis*, the ſtricture of the prepuce occaſions a prodigious ſwelling of the *glans*, though a greater afflux of arterial blood cannot be ſuſpected in either caſe.

But all the *muſcles* are irritable. I do not know one that has not a natural palpitation after death; they all tremble, and are alternately contracted and relaxed. I have obſerved it in the temporal, pectoral, and ſternocoſtal muſcles, in the *recti abdominis*, *cremaſter*, and *ſphincter* of the *anus*. Dr. WHYTT [t] has obſerved the ſame in the *ſphincter* of the bladder, and other anatomiſts in other muſcles of the body. With regard to the ſternocoſtals, I have often ſeen with pleaſure, that upon cutting away the *ſternum*, they ſtill preſerved a ſufficient force to bend the cartilages of the ribs, and pull them inwards. The ſame muſcles preſerved their Irritability a long time, nay even longer than the diaphragm. Farther, as the fleſh in general of animals palpitates ſpontaneouſly after death, ſo it appears from an old experiment, which is known to every body [u], that this motion may be rvived after the muſcles have been ſome time quiet, either by irritating the nerve which goes to a muſcle, or the muſ-

[t] P. 93. [u] HIGHMOR, *diſquiſ. anat.* p. 137. B. LANGRISH *de mot. muſc.* p. 51. WOODWARD p. 74. l. c. PARSONS *de mot. muſc.* p. 68. W. CROONE *de mot. muſc.* p. 10. MAZINI *de mechan. medic.* p. 13. HUGHES *of Barbados* p. 309.

cle

cle itſelf, with a knife, or corroſives. Dr. ZIMMERMAN[x] has made experiments of the ſame kind with mine, as alſo Dr. WOODWARD[y] upon the muſcles of oxen, CROONE[z] upon a muſcle of the human thigh, and BREMOND[a] upon a frog; and OEDER[b] obſerved the muſcles to be violently convulſed upon being touched with ſalt. In making the former experiment it is of little conſequence, whether the nerve is intire and communicates with the brain, or whether it has been cut through[c]. In both caſes the muſcular fibre contracts, its extremities approach nearer to one another, and the ſucceſſion of theſe motions repreſents a kind of undulation on the ſurfaee of the muſcle. On examining in a frog with a microſcope the muſcle thus convulſed, you do not ſee that the blood is puſhed out; and it circulates as before. There is no animal in which the muſcles grow pale during their action, and I have long ago obſerved, that the paleneſs which HARVEY[d] ſaid he had obſerved in the heart during its contraction, has been a ſource of errors into which the greateſt men have fallen.

In moſt part of the muſcles the Irritability is ſo great, that after one ſingle irritation, the muſcle contracts and is relaxed ſeveral times, by oſcillations which diminiſh gradually, till they intirely ceaſe[e]. This alternate motion is very

[x] P. 19. [y] P. 73, 74, 75, 76. [z] *De mot. muſc.* p. 30. [a] Mem. de l'Acad. des Sciences 1739, p. 476.
[b] P. 2. [c] CL. OEDER p. 5. [d] *Comment.* BOERH. *n. cordis* 400. *Phyſ. prim. lin. n.* 4. [e] WHYTT pag, 18.

evident

evident in the *recti* muſcles of the *abdomen*, the ſternocoſtals and others, where there is no difference in the direction of the fibres, which therefore HAMBERGER[e], and another author, had no manner of occaſion to introduce into the ſtructure of the heart, ſeeing the muſcles juſt now mentioned oſcillate perfectly well, though all the fibres are ſtraight and parallel to one another. In the mean time Dr. WHYTT[f] is miſtaken when he ſays, that this oſcillation takes place in all the muſcles, for it does not happen in the bladder, which after it begins to act, continues its contraction without interruption till all the urine is expelled.

The *iris*, which you will be ſurprized at, has no Irritability, when the cauſe of the irritation applied to it is mechanical, *viz.* a knife or the like, as I ſaid before. Now that I mention the *iris*, I have obſerved, contrary to Dr. WHYTT's[g] opinion, that its dilatation does not depend upon muſcular force, becauſe after death it remains very large. This I had remarked before a great many times, and I have a proof of it before me in a cat which was tortured to death, and has the pupil opened ſo wide, that you can ſcarce ſee any part of the *iris* at all. In a frog it alſo appears to be void of Irritability.

There are ſome muſcles which have a ſtronger contracting force than others, and retain it a longer time after the animal is dead. The chief of theſe I reckon the *diaphragm*, which I have always obſerved to continue to move a long while

[e] In progr. *de cauſa dilat.* [f] Pag. 243. [g] Sect. 7.

after

after the others when the animal was dead, or at leaſt by irritating the nerves the motions could be renewed. I have ſeen it irritable and tremble an hour or more after death, when the motion of the inteſtines had intirely ceaſed; and Dr. ZIMMERMAN[h] has been witneſs of the ſame along with me. WEPFER[i] has obſerved long ago, that the diaphragm contracted after the ſtomach was cut out. At the ſame time I muſt own, that I have ſometimes ſeen in animals while they were ſtill warm, other muſcles, and the œſophagus continue their palpitations, after the heart was perfectly quiet, an example of which is related by OEDER[k]. But for the moſt part the diaphragm, heart and inteſtines may be irritated, or the heart and inteſtines palpitate ſpontaneouſly, after the motion of all the other parts is quite ceaſed.

The *œſophagus*, if it is irritated above the diaphragm, contracts itſelf very ſenſibly, and I have plainly ſeen its periſtaltic motion without any irritation, and a morſel contained in it puſhed upwards and downwards, by the periſtaltic motion renewed upon irritation, which I think is ſufficient to refute the opinion lately advanced againſt the motion of this muſcular tube.

The *ſtomach* is conſiderably irritable, and when touched with a corroſive, its impreſſion immediately produces a long ſuperficial furrow in it.

[h] P. 19. [i] *De cicut. aquat.* p. 195. [k] De temporali p. 4.

If you irritate it with a knife, either at the *pylorus* or elſewhere, it preſently contracts; and I have ſeen it, eſpecially upon touching the left ſide of the *pylorus* with a corroſive, contract itſelf circularly. If after you have opened it, you touch it with a corroſive, it throws out a froth, and the borders of the wound are rolled together *, as do thoſe of the inteſtines. I have likewiſe ſeen the ſtomach ſhining through the diaphragm while the *abdomen* was intire, agitated with a periſtaltic motion, and the ſame may be obſerved upon viewing it through the *peritonæum* after it has been laid bare, which contradicts the opinion of SCHWARZ, who attributes that motion to the acceſs of the external air. I have ſeen it very evidently in a cat, a rabbit, and a rat, continue more than an hour after that of the inteſtines had ceaſed.

There is however in the ſtomach ſomething leſs active, compared with the inteſtines; and upon irritating it in a frog with corroſives, it does not at all contract. I have frequently given theſe ſubſtances to them, and never but once ſaw them produce thoſe motions which occaſion vomiting, and conſiſt in ſtrong and ſhort ſuccuſſions repeated from time to time. Another time I ſaw the corroſive ſublimate conſtrict the ſtomach, and make it perfectly flat.

The *inteſtines* both large and ſmall, and the *cæcum* in thoſe animals where it is large, are extreamly irritable; for after having cut the muſ-

* Et revolvit labia vulneris.

cles of the abdomen entirely away, I have ſeen the excrements driven out by the force of the inteſtines alone, as WEPFER and STHAL[l] have obſerved before. We may add to theſe facts, ſo contrary to the ſyſtem of thoſe who attribute too much to the contraction of the abdominal muſcles, that in the caſe of an obſtinate coſtiveneſs, when the excrements reſiſt the moſt violent ſtraining, and repeated efforts of reſpiration, there is often nothing more required to expel them, than rouſing the Irritability of the inteſtines by a gliſter. There is no part in the animal body which continues longer to move, frequently longer than the heart, as I have obſerved fourteen times; and when the heart moves the longeſt, it appears to me to be owing to the *abdomen* being firſt opened, and the inteſtines thereby rendered cold[m]. Nevertheleſs the briſk and perpetual motion of the heart, together with other reaſons, give it the preference to the diaphragm. Opium, which deſtroys the periſtaltic motion of the inteſtines, and almoſt the whole Irritability of the body, leaves the force and motion of the heart intire, as I have frequently ſeen. In ſeveral experiments, the motion of the heart has continued longer than that of the inteſtines, whereof I find ſeven examples in the journal of my diſſections.

Very often after the motions of the inteſtines have ceaſed, they are renewed again, and in-

[l] Theor. vit. & mort. [m] Comp. OEDER p. 5. & I. G. ZIMMERMAN.

creaſed by little and little, either by the cold or ſome obſcure cauſe irritating them. Beſides, after they have been taken out of the body, I have obſerved this motion rather to increaſe, which according to the common received opinion ought to be totally deſtroyed, and Dr. FELIX[n], my pupil, has already made this remark. They may likewiſe be irritated externally either with a knife, a needle, alcohol, or corroſives; but their internal ſurface is much more irritable. Upon opening the inteſtinal canal, and dropping a corroſive into its cavity, the byle deſcends and flows out with a great deal of froth, and is alternately reſorbed into the inteſtine. I never ſaw the periſtaltic motion more plain than in a cat, which had ſwallowed corroſive ſublimate. The lips of the inteſtine where it is cut through turn back, ſo as to embrace the part immediately above the ſection, and thus being turned inſide out, readily adhere to the neighbouring parts. When the inteſtine is only ſlightly wounded the lips are equally retracted.

But it is ſo difficult to obſerve the periſtaltic motion, that it is very hard to reduce it to any certain rule. In general however the inteſtines are evidently conſtricted where that motion obtains, while the part below the conſtriction is dilated, and receives the contents which the conſtricted part ſends to it. Thus, when a part of the inteſtine is irritated, it contracts ſo ſtrongly there, that the cavity is quite ſhut up, and the contents are puſhed into the neighbouring parts, either up-

[n] De motu periſtaltico n. 11.

wards or downwards, which dilate, and ſoon afterwards, in conſequence of this dilatation, contract themſelves, and puſh along whatever is contained in them.

I have ſeen an introſuſception in a rabbit, which had poiſon given it. In this caſe the contracted part of the inteſtine is received into the neighbouring part which is larger, and it is eaſily diſentangled; but in the mean time it ſqueezes the contents equally upwards and downwards. It is likewiſe certain, that the inteſtine changes its ſituation from ſide to ſide, moving itſelf alternately from the left to the right, which motion renders the longitudinal fibres very conſpicuous, as the conſtriction does the tranſverſe.

In frigid animals the inteſtines appear to me to be proportionably leſs irritable. An hour after having opened the belly of a frog, I ſtill found the ſtomach and the inteſtines to be irritable, but the motion of the heart continued a good deal longer.

Thus by degrees I am at laſt got to the moſt irritable organ of all, *viz.* the heart, which as it is the cauſe of all motion in the human body, ſo is the beſt conſtructed for it, and therefore ought to be endowed with the greateſt Irritability. That it is actually ſo, appears from experiments, eſpecially in frigid animals, in which it is conſtantly irritable, and much more than the inteſtines. For firſt, in theſe animals it moves a great deal longer than any other part of the body, even after death, and ſometimes for

four and twenty, [o] or thirty [p] hours, or longer; but in animals whoſe blood is hot, it moves till the fat is rendered ſtiff by the cold, at which time the motion of all the muſcles commonly ceaſes. I have obſerved in frogs, that generally the heart continues its motion from noon till pretty far in the night, but rarely till next morning. Farther, after the heart has ceaſed to move, its motion may very eaſily be renewed by external irritation, either with a knife, a needle, ſalt [q], poiſon, and ſometimes even with ſimple hot water, according to Dr. WOODWARD [r]. The auricle being irritated by poiſon, contracted itſelf ſeveral times ſucceſſively, and I have ſeen the ſame thing happen in the heart. But very often in thoſe irritations produced by poiſon, the motion which follows upon them is very ſhort, oft enough local, and confined to the place alone which is irritated. The beſt method of renewing the motions of the heart, is to irritate its internal ſurface, and I have frequently ſucceeded by blowing into it, after having tried corroſives without any ſucceſs; and the injection of other fluids, which have more conſiſtence than air, produces the ſame effect. The motion of the heart is equally renewed, whether you inject water, or blow air into it, either by the *vena cava*, the *trachea arteria*, or the thoracic duct [s], which laſt experiment I tried upon a dog.

[o] In a large viper CHARAS *de la theriaque* p. 43. [p] In a turtle 1. CALDESI. [q] OEDER p. 3. [r] L. C. p. 52. [s] WEPFER cicut. aquat. p. 29.

In

In ſhort, it is ſufficient if the air gets any how into the left ventricle, which experiment uſually goes by the name of HOOKE's, and I have tried it myſelf frequently with ſucceſs. This irritation of the internal ſurface of the heart, produces oſcillations much more laſting, than thoſe which are cauſed by irritating it externally, and they become weaker very inſenſibly. Neither does this irritation at all diminiſh the Irritability of the heart, whereas that which is produced by poiſons, renders whatever part they touch almoſt incapable of motion.

It is difficult to determine which is the moſt irritable part of the heart. Anatomiſts have commonly preferred the right ventricle and auricle. But, if I am not miſtaken, I have proved that in this reſpect the right has no advantage over the left, ſeeing the oſcillations of this laſt continue longer, when the irritating cauſe has been applied longer to it than to the right[t]. It does not appear that the weight of the liquor which is made uſe of contributes to increaſe the irritation, becauſe air produces the ſame effect as water, although it is near a thouſand times lighter, and the heart of a *fœtus* beats ſtronger and quicker than that of adults, whoſe blood is a great deal thicker and heavier. Hence I conclude that the different weight of the blood has no great influence upon the motion of this organ; and the experiments juſt now mentioned, made with air and water, prove that there is no need

[t] See the eſſay publiſhed at the end of this treatiſe.

of acrimony in the fluids to produce an irritation of the heart; though at the ſame time I muſt obſerve that acrimony increaſes it, as appears from the example of ſalt. But acrimony and irritation do not increaſe in the ſame proportion; for hot ſpirit of nitre, applied to the internal ſurface of the heart, produces no contraction.

If you aſk me whence proceeds this greater Irritability of the heart than of the other muſcles, I ſhall find it very difficult to anſwer the queſtion. The nerves are not more numerous there than elſewhere, and they are rather ſmaller than thoſe of the muſcles of the eye. Dr. WHYTT[u] is of opinion that the nerves there are more ſenſible, and therefore can leſs reſiſt any ſtimulus. But whence can this exceſs of ſenſibility in the heart proceed? Isit becauſe the nerves are more expoſed, and nearer the internal ſurface of the heart, and by that means nearer the irritating cauſe? Anatomy gives us but little light into this ſubject, unleſs we might take for example the auricles, which are both very thin and very irritable. What would induce me to adopt this hypotheſis, is the great Irritability which is obſerved in the inteſtines, which have only few nerves, but theſe very much expoſed. To convince ourſelves how much this circumſtance increaſes the ſenſibility, we need only examine the ſymptoms which ariſe, when the mucus of the bladder and *urethra* is abraded; and of the inteſtines, when upon being de-

[u] Pag. 311.

prived

prived of their mucus, their naked villous membrane oozes out blood. But it is difficult to ſupport this ſyſtem by anatomical facts; for ſo far from being able to demonſtrate, that the laſt ramifications of the nerves are extremely expoſed in the heart, we find it difficult to diſcover their principal trunks. Beſides, of all animals an eel appears to me to have the heart and other muſcles the leaſt capable of irritation.

From all theſe experiments collected together it appears, that there is nothing irritable in the animal body but the muſcular fibre, and that the faculty of endeavouring to ſhorten itſelf when we touch it is proper to this fibre. From the ſame experiments it likewiſe follows, that the vital parts are the moſt irritable; the diaphragm frequently moves after all the other muſcles have ceaſed, the inteſtines and ſtomach move ſtill longer, and laſtly the heart continues its motions after all the other parts are quiet. This furniſhes us with a diſtinct character between the vital organs and the others, *viz.* the firſt, being extremely irritable, require only a weak ſtimulus to put them in motion, whereas the others, which are endowed with very little Irritability, are not to be moved but by the determinations to the will, or by very ſtrong irritations, the application of which is capable of producing in them violent commotions, known by the name of convulſions.

I ſhall eaſily prove, that this power of producing motion is different from all the other pro-

perties

perties of bodies[x]. Elaſticity, which ſeems to have the moſt reſemblance to the other, differs from it in this, that it is proper to dry fibres, in which ſtate they are deprived of all Irritability, as eaſily appears upon drying a frog. Beſides elaſticity is the property of hard bodies, and Irritability of the ſofteſt. The polypus is ſo irritable, that though it has no eyes, the light affects it very ſenſibly. Gelatinous animals are extremely irritable, but far from being elaſtic. Dr. WHYTT[y] adds, that the motion of the heart ceaſes and is renewed ſpontaneouſly, which is not obſerved in any elaſtic fibre, and upon pricking ſteel with a needle you produce no irritation in it. Dr. BATTIE[z] has obſerved, that Irritability is leſs conſiderable in old ſubjects than in young, although the fibres of the former are more elaſtic than of the latter.

But the muſcular fibres being compoſed of earthy particles and a glutinous mucus, it may be aſked in which of theſe Irritability recides. It appears moſt probably to recide in the latter, becauſe this when it is pulled endeavours to ſhorten itſelf; whereas on the contrary, dry earth never changes its figure of itſelf, and being extreamly brittle, when its parts are ſeparated, they conſtantly remain ſo. This opinion is ſtrengthened by obſerving that children, in whom the mucus predominates, are much more irritable than adults; which is evidently proved from the quick-

[x] ZIMMERMAN in addend. OEDER p. 7. [y] P. 231, &c.
[z] De princip. anim. p. 34.

neſs

neſs of their pulſe, which vibrates one hundred and forty times in a minute, whereas in old perſons the vibrations are not above ſixty or ſixty-five in the ſame ſpace of time. Farther, the moſt ſolid and earthy parts of our body, *viz.* the bones, teeth and cartilages, are void of Irritability, and the moſt irritable parts are deprived of that quality, only by robbing them of their mucus by drying.

We ſhould next inquire how this mucus, produced from inſenſible lymph, can become irritable. Dr. Whytt, and the followers of Sthal, alledge that it is owing to the ſoul, which being ſenſible of the touch, contracts the fibres that are touched, and pulls them back, to prevent their being injured.

However ſimple this theory may be, and however commodious for diſembarraſſing us from ſeveral difficulties, yet it is not agreeable to the phænomena which are obſerved. For in the firſt place Irritability differs intirely from ſenſibility, and the moſt irritable parts are theſe, which are not ſubject to the command of the ſoul, which ought to be quite the reverſe if the ſoul was the principal of Irritability. In the ſecond place, Irritability continues after death, and parts quite ſeparated from the body, and intirely inſenſible, after the heart is taken out, and the head is cut off, remain ſtill irritable. There is nothing more common than to ſee the heart of a frog beat, and the muſcles remain irritable, after the head has been cut off, and the ſpinal marrow taken

out.

out. Dr. WHYTT[a] ſhuns this difficulty with great addreſs by ſaying, that the time of death is very uncertain, and that frequently an animal has life ſtill remaining, after it has been looked upon as dead for ſome time; and this he proves by the example of perſons who have been drowned, and thoſe who fall into *ſyncopes*. But as it is certain that the ſeat of the ſoul is in the head, and that it has no command over the reſt of the body, after the nerves have been cut or deſtroyed; and farther, as the Irritability remains intire after the head is lopped off, or the nerves cut through, it appears that this quality ſtill ſubſiſts after the ſeat of the ſoul is removed, or its commerce with the body quite intercepted, and therefore it does not depend upon the ſoul. This is ſo evident that it is needleſs to add, that Irritability acts without the ſoul being ſenſible of it, and that it is not ſubject to the command of the will, both which are proved by the example of the heart. To avoid the conſequences of this argument, the anatomiſts are obliged to introduce an inſenſible ſenſation, and involuntary acts of the will, that is to ſay, to admit contradictory propoſitions.

What therefore ſhould hinder us from granting Irritability to be a property of the animal *gluten*, the ſame as we acknowledge attraction and gravity to be properties of matter in general, without being able to determine the cauſe of them. Experiments have taught us the exiſtence

[a] P. 367, 389, &c.

of

of this property, and doubtleſs it is owing to a phyſical cauſe which depends upon the arrangement of the ultimate particles, though the experiments that we can make are too groſs to inveſtigate them.

Irritability is deſtroyed by drying, by the congealing of the fat, but eſpecially by the uſe of opium in a living animal. This medecine deſtroys ſo effectually the periſtaltic motion of the inteſtines and ſtomach, that it cannot be revived again by any irritation. I have ſeen it myſelf, and Dr. KAAU BOERHAAVE [b] has obſerved the ſame thing before. Once however in a cat I found opium not capable of ſubduing the periſtaltic motion. It has the ſame efficacy in deſtroying the periſtaltic force of the bladder; nay in a frog I have ſeen it deſtroy the periſtaltic motion and irritability of the inteſtines, and the convulſive force of the nerves. Dr. WHYTT ſays, that it likewiſe deſtroys the Irritability of the heart, but that I could never diſcover [c].

To conclude, Some celebrated authors having aſſerted that Irritability was a property unknown till now, and having given me the honour of the diſcovery, while others have neither looked upon it as new, nor really exiſting, I have thought proper to add a ſhort hiſtory of it here. Some obſcure experiments which offered themſelves naturally have been known

[b] In impetum faciente HIPPOCRATICO.

[c] P. 371, 372.

in all ages, and VIRGIL has obſerved the trembling of the fleſh cut off from the body immediately after death, but I don't ſee that the ancients have tried any experiments to irritate the fibres and renew their motions. FRANCIS GLISSON[d], who diſcovered the active force of the elements of our bodies, was the firſt who invented the word *Irritability*, which he attributes to a *natural perception*, that is not accompanied with any ſenſation, and depends upon Archæus who is the framer of his own body[e]. He diſtinguiſhes it into two ſorts, the one of which depends upon the external ſenſes, and the other upon the internal appetite[f]. He likewiſe mentions ſome facts to prove, that this motion is produced independent of ſenſation, that after death the fibres contract upon being touched with acrid and pungent liquors; nay he makes this property ſo general, as to allow even the bones and human juices to be irritable[g]. He likewiſe diſtinguiſhes the different degrees, and amongſt the reſt that exceſſive Irritability, which BOERHAAVE called *prurient*[h].

BELLINI[i] mentions a *natural contraction*, and explains mechanically how acrid bodies, which might irritate the fibres, are repelled by means of this property, and thence he accounts how irritating ſubſtances may occaſion contractions of the muſcles, accelerate the motion of the

[d] De ventriculo & inteſtin. c. vii. [e] N. 6. [f] N. 11. [g] C. 8. n. 1. [h] *Ib.* n. 6. [i] de *Stimulis* amongſt his *opuſcula* and in his book *de ſanguinis miſſione*.

blood,

blood, occaſion an inflammation, produce a revulſion, or ſome certain evacuation. But he does not confirm theſe opinions by any experiments. BAGLIVI[k] by his experiments has approached nearer to a diſcovery of it. He obſerved pieces of the heart without the aſſiſtance of any nerve, tremble and oſcillate, and preſerve their alternate motion of contraction and relaxation[l]; and he has remarked, that the muſcular fibres contract themſelves upon being touched, without the ſoul's having any ſhare in it, or even being ſenſible of it[m].

Since that time all the followers of STAHL have enlarged much upon the tone and natural contraction of the fibres, but they attribute it to the ſoul, and as they have always had an averſion to anatomy, they have made no experiments to illuſtrate this quality.

Dr. BOERHAAVE[n] has acknowledged an active force in the heart, and a latent principle of motion in the pieces of it which are cut; but as he attributes the cauſe of muſcular motion to the nerves, this proves that he did not ſufficiently know, that the cauſe of this motion was in the muſcles themſelves, that the nerves had no other office but to ſubject them to the will of the ſoul[o], and that they could indeed increaſe or diminiſh it, but were not the cauſe of it, becauſe it is extended far beyond the nervous ſyſtem, and we meet with it in in-

[k] *De fibra motrice & morboſa.* [l] P. 7. [m] P. 12.
[n] Inſtitut. rei med. n. 187. not. 1. 11. [o] *Ib.* n. 402.

ſects

ſects which have not even a head. We find a number of curious experiments upon this ſubject, in Dr. WOODWARD's poſthumous ſupplement, publiſhed by HOLLOWAY. Dr. STEWART[p] has likewiſe obſerved ſeveral very uſeful facts, and he formerly remarked, that the fibres preſerved their irritability after the nerves were detached from them.

We find ſcattered in ſeveral other authors, a good many things relating to this ſubject, which ſeem rather to have been dropt accidentally than wrote on purpoſe.

In my commentaries upon BOERHAAVE's[q] Inſtitutions, publiſhed in 1739, I have expreſſed myſelf as follows, *Wherefore the heart is moved by ſome unknown cauſe, which neither depends upon the brain nor the arteries, but lays concealed in the very ſtructure of the heart itſelf.* The nature of the thing obliged me to differ in opinion from my preceptor. Three years afterwards I publiſhed the following doctrine[r], *viz. That all animal fibres when they were irritated contracted themſelves, that this character diſtinguiſhed them from thoſe of vegetables, and that the perpetual irritation alone was the cauſe of the continuation of motion in the vital organs, while the animal organs ceaſed to act.* In my abridgement of the phyſiology[s] I have poſitively aſcribed the motion of the heart to the force of

[p] De mot. muſcul. p. 13. [q] Ad. n. 187. Inſtit. rei med not. i. 11. [r] Tom. 4. pag. 586. ann. 1743. [s] Ann. 1747. n. 113. pag. 51.

a *ſtimulus*, and in the ſecond edition, I have been more explicit on the Irritability of the muſcular fibres[t], aſſerting that it was independent of the nerves, and of every other known property. If any perſon denies the truth of this aſſertion, I ſhall be glad to be informed by him, what the property is upon which that motion depends. Since that time, numerous experiments made upon living animals, had convinced me of the truth of the doctrine above advanced.

It gave me no ſmall pleaſure to ſee, that almoſt at the ſame time Dr. DE GORTER[u] publiſhed ſeveral obſervations to the like purpoſe, and that the celebrated Dr. WINTER[x], phyſician in ordinary to the houſe of Orange, in an oration *de certitudine in medicina practica*, attributed all the motions of the human body to the Irritability of the fibres, and the force of a *ſtimulus*. Theſe two famous phyſicians have been followed by ſeveral others. Dr. KAAU[y], nephew to the great BOERHAAVE, has made a great number of experiments, although with another view; and not long ago the celebrated Dr. WHYTT[z] has attributed all the motions of the human body to the force of a *ſtimulus*; but with this difference between him and the others, that he imputes Irritability to the ſoul, which feeling the impreſſion of the irritation, occaſions

[t] N. 408, p. 252. [u] Exercitat. de motu vitali.
[x] Franeker 1746. fol. [y] De impet. facient HIPPOCRAT. [z] Of vital motions, Edinburgh 1751. 8.

the contraction of the fibre. I cannot help complaining of this gentleman's want of candour, both with regard to myself, and to several others, seeing he never mentions me but when he wants to criticise me, and has adopted several of my ideas, without mentioning whence he had them. He has made but a small number of experiments upon dying animals, which he has published with a view to support his system, but they have not been often enough repeated to be established as certain, and some of them are contradicted by those which I have made.

Two of my pupils, Drs. ZIMMERMAN and OEDER, have hit upon the proper method of arriving at the knowledge of this property, both these gentlemen endeavouring by experiments to explain this quality of a fibre, as a law of nature resembling attraction, without entering into useless researches in theory. CASTEL has likewise confirmed the experiments upon sensibility. The deceased M. DE LA METTRIE[a] has made Irritability the basis of the system which he advanced against the spirituality of the soul; and after saying that STAHL and BOERHAAVE knew nothing of it, he has the modesty to assume the invention to himself, without ever having made the least experiment about it. But I am certainly informed, that he learnt all he knew about it of a young Swiss with whom I am not acquainted: who never was my

[a] L'Homme machine n. 18. 22.

pupil,

pupil, nor is he a phyſician, but he had read my works, and ſeen ſome of the famous ALBINUS's experiments, and upon theſe LA METTRIE founded his impious ſyſtem, which my experiments totally refute. For if Irritability ſubſiſts in parts ſeparate from the body, and not ſubject to the command of the ſoul, if it reſides every where in the muſcular fibres, and is independent of the nerves, which are the *ſatellites* of the ſoul, it is evident, that it has nothing in common with the ſoul, and it is abſolutely different from it; in a word, that neither Irritability depends upon the ſoul, nor is the ſoul what we call Irritability in the body.

SUPPLEMENT by the AUTHOR.

Having ſeen, ſince the publication of my eſſay on Irritability, M. LE CAT's objections to what I had advanced, in a paper which he ſent to the Royal Academy at Berlin[b], I thought it incumbent upon me to anſwer it in a few words.

I do not know what motive could engage this author, or M. DELIUS, to refute me before I had publiſhed my opinion. They have either laid hold of ſome *theſes* of my pupils, or expreſſions which have eſcaped me in ſome letter to a friend. This is certainly the caſe with M. LE CAT, and both of them might

[b] At the end of an eſſay upon muſcular motion.

have ſpared a great part of their criticiſm, if they had only waited till the publication of my treatiſe.

In my firſt eſſay upon this ſubject, I inquired if the *dura mater* or tendons were irritable, if they contract, when any external violence is applied to them, and if in effect they can act like muſcles. This enters eſſentially into BAGLIVI's ſyſtem, and I plainly aver the contrary. All the animals which I have examined, have the *dura mater* ſtrongly attached to the *cranium*, and when it is detached from it, it is always void of motion. In vain do we attempt to irritate it with a knife, needle, or corroſives more or leſs gentle; there is thereby no commotion raiſed in the animal. The ſame is the caſe with the *pia mater* likewiſe. In M. LE CAT's experiment, the application of ſpirit of wine is ſcarcely felt in the *dura mater*, whereas it produces a very violent pain in the ſkin; which ſhews that the firſt has no ſenſibility when compared with the ſecond. Upon irritating the *medulla* of the brain, and that of the ſpine, convulſions are thereby quickly produced; wherefore the cauſe of motion is in the *medulla*, and the *meninges* have nothing at all to do with it.

The ſecond doctrine which I have advanced is this, *viz.* that wounds of the *perioſteum*, tendons, ligaments and *dura mater*, do not hurt the animal, and are cured without any accident. The experiments which M. LE CAT has recourſe to againſt me are of no ſignification.

They

They are vague and inconcluſive. He muſt produce patients where a tendon, ligament, or one of the *meninges*, has been indiſputably and ſingly wounded, and where troubleſome accidents have enſued thereupon. What he ſays of the *dura mater*[c], is explained by the compreſſion which is produced in the brain, in conſequence of that of the *meninges*. When we ſeparate the *dura mater* from the *cranium* with the finger, the animal complains, even a moderate compreſſion of the brain makes it ſuffer, and if it is pretty ſtrong, the animal falls aſleep. In the patient named CLERMONT, whom M. LE CAT[d] mentions, the optic nerve was hurt, as he himſelf owns, and it is very difficult in a curſory diſſection to know, whether in the one called COURVET, the nerves of the eyes, and eſpecially thoſe that creep in the bottom of the orbit, towards the temples, were really preſerved. The ſpaſm may have cauſes not to be penetrated into by our ſenſes, and founded upon the moſt delicate ſtructure of the nerves. The hyſteric *tetanus*, and that in animals which have been poiſoned, confirms this opinion, and the obſervation of M. LE CAT[e] proves abſolutely nothing at all, ſeeing it does not exclude this diſorder, which is too deep ſeated to be viſible. The hiſtory of PERCHEPIE[f] ought not to be produced againſt me, ſeeing this patient had *pus* in the ventricles and under the *baſis* of the brain, which is more than ſufficient to occaſion

[c] P. 113. [d] P. 115. [e] P. 118. [f] P. 119.

a *delirium*. In order to refute me, M. LE CAT muſt bring experiments ſuch as mine; the *dura mater* irritated, and convulſions thereby produced; the tendons pierced and wounded, and the ligaments pricked or burnt, and followed by violent ſymptoms. But no ſuch experiments can be produced; nature is too conſtant, and I have ſeen her act too often to be deceived in this point. Neither ſhould the difference between man and brutes be here quoted againſt me; for if wounds of the tendons had any influence upon their motion, a kid, a rabbit or a dog would not be able to leap up on a chair, immediately after the *tendo achilles*, had been pierced, cut, or deſtroyed[g].

Laſtly, I have ſaid, that the tendons, *periofteum* and *dura mater* are all inſenſible. I am not the very firſt who has advanced this truth, and I have quoted obſervers, who having no ſyſtem to defend, had remarked the ſame thing before me. M. LE CAT mentions no experiments againſt me here, but with regard to the *dura mater*[h]. He relates that one, named FLEURI, complained upon his preſſing this membrane with a crotchet; and that another wounded patient, called MABIRE[i], was ſenſible of the motion of a tooth-pick upon the *dura mater*, that he felt the application of ſpirit of wine[k], and the action of the ſurgeon who waſhed the wound[l]; and conſequently that the *dura mater* muſt ei-

[g] See the thetis of Dr. CASTEL. [h] P. 122.
[i] P. 124. [k] P. 129. [l] P. 135.

ther

ther be almoſt cartilaginous, or oſſified, in thoſe ſubjects which do not ſeem to have any ſenſation in this membrane. It even appears by his own expreſſions, that he has ſeen examples of this inſenſibility[m].

A thouſand times have I ſcratched, burnt, and cut the *dura mater*, in I don't know how many different animals, without their ever complaining, and they have ſeemed to feel ſtill leſs the ſpirit of wine, which is infinitely leſs active than the butter of antimony or ſpirit of nitre. Young animals have ſucked, or lapt milk, during the tearing of this membrane, without ſeeming at all ſenſible of what was a doing. It is abſolutely impoſſible to imagine that the *dura mater* ſhould be cartilaginous, or almoſt bony, in young healthy animals. The ſame animals were very ſenſible when the ſkin was pinched or pulled, which they diſcovered by making a noiſe, and endeavouring to pull away the part. The experiment has been made upon animals fierce and impatient, ſuch as cats for example, which become furious when they find themſelves in danger and pain, nay, the ſame experiment has been tried upon man, and M. Eller has confirmed the truth of this doctrine at Berlin, upon the *dura mater* of a man, which had been laid bare by a *caries*. If the wounded perſon mentioned by M. le Cat felt the preſſure, it was no more than brutes themſelves are ſenſible of; they feel

[m] Page 129. near the bottom.

very well the detaching of the *dura mater*, and the finger reſting upon it, as I juſt now remarked. It might not even be impoſſible for very penetrating remedies to act beyond the *dura mater*, as cold water and acids affect the nerves of the teeth, through the nervous and bony ſubſtance which intervenes. But I have never obſerved this fact to happen, and I repeat it once more, that the *dura mater* being only a cellular membrane, becoming evidently ſuch where it accompanies the nerves, and having no nerves itſelf, it cannot be ſuſceptible of any ſenſation.

I have only one word more to add, which is, to entreat phyſicians and ſurgeons, to lay hold of all opportunities of inſtructing themſelves with regard to the inſenſibility of the *perioſteum*, tendons, ligaments, and membranes of the brain. Such opportunities muſt frequently enough occur, and after they have laid hold of a tendon with a pair of *forceps*, as I have done that of the *flexor* of the third articulation of the finger, they will find themſelves emboldened to make experiments, which are neither attended with danger nor inconveniency to the patient.

Allow me to add here that M. LA MURE publiſhed in tome 1749, of the *Memoirs of the Royal academy of arts and ſciences at Paris*, an eſſay concerning the motion of the brain, analogous to that of reſpiration, which was read the 7th of Auguſt 1752[m]. This of mine was read before

[m] P. 541.

the

the royal ſociety at Gottingen, the 22d of April and 6th of May 1752. Hence you ſee that it was publiſhed previous to M. LA MURE's experiments, and it is plain that mine are anterior to his, ſeeing they are quoted by him[n], and I was entirely unacquainted with thoſe which he had made. Envy takes a pleaſure in laying hold of ſlight occaſions of detraction, and an eſſay wrote in 1752, but publiſhed in the volume of 1749, might furniſh occaſion for it in this caſe; and it would not be agreeable to be thought to have copied afer another, when a perſon has followed nature alone. BERN 10th *Sept.* 1754.

[n] Memoirs of the academy 1749, page 542. I communicated them in a letter wrote to M. DE SAUVAGE the beginning of January 1752, who ſent me an anſwer as follows, dated March 1, 1752. *The dog was trepanned, and we obſerved a very conſiderable motion of the brain, exactly like that which you deſcribed in your letter to me. In order to be certain whether it is the reflux of the blood which cauſes this elevation during the time of expiration, M.* LA MURE *opened more than ten dogs: In ſhort we have found the caſe to be exactly as you deſcribe it, and I am extremely obliged to you for communicating this diſcovery.*

AN

AN

ESSAY

ON THE

CAUSE of the MOTION

OF THE

HEART.

Read November 10th, 1751*.

ALthough this eſſay is but ſhort, yet it will have its uſe; ſeeing you will find an experiment in it which I have made ſeveral times, and which proves that the motion of the

* Although this eſſay is independent of that which was read before, and even appeared ſooner; yet as it contains experiments upon the Irritability of the heart, which determine the cauſe of theſe motions, I imagine it will be agreeable to the reader to ſee it ſubjoined to the preceding, ſeeing both may be conſidered as pieces belonging to the ſame ſubject, which reciprocally eſtabliſh each other.

heart,

heart, by perpetual alternate contractions and relaxations, depends upon irritation, occaſioned by the veinous blood which is ſent thither. All the explanations which have hitherto been given of this phænomenon, are deſtroyed by the human or comparative anatomy.

We know perfectly well that the right ventricle, and eſpecially its *auricle*, are the laſt parts of the body which preſerve any motion. GALEN [o], HERVEY [p], and BOERHAAVE [q], were convinced of this by experiments.

I have long ſuſpected [r], that the continuance of this motion depended upon the blood, which the *venæ cavæ*, contracted by the cold, and compreſſed by the palpitations and weight of the muſcles, ſend continually to this ventricle; whereas the lungs of the dying animal, being motionleſs and collapſed, no longer admit the blood by the pulmonary artery; and that all the quantity which its contraction can force into the left auricle, is too inconſiderable, compared with that which returns from the whole body to the right auricle, to produce a ſenſible effect. It may therefore be affirmed, that if the right ventricle and auricle, move longer than the left auricle, it is becauſe the veinous blood is ſent longer thither.

I determined to confirm my conjecture by experiments, and in order to this, it was ne-

[o] Admin. anatom. lib. 7. c. 15. [p] Diſs. 1. p. 39. 44, &c. [q] Inſtit. rei med. No. 159. [r] Commentar. in BOER. c. 4. p. 609, prim. lin. phyſ. No. 113.

ceſſary,

ceſſary, if poſſible, to prevent the entry of the blood into the right ventricle, and if by this means its motions were ſtopt, that was a proof that they actually depended upon the acceſs of the blood.

I attempted this experiment at firſt by ligatures, becauſe I remembered to have read in BARTHOLIN[s], and BERGER[t], that the tying of the veins makes the motion of the heart ceaſe, and that it begins again when the ligature is cut: and HERVEY ſays that he has made the ſame experiment upon a ſerpent[u].

But the experiment done in this manner did not ſucceed, becauſe as long as the animal is ſtill warm, the blood contained in the right auricle continues to move it, though there is none ſent to it by the *venæ cavæ*; and after having tied them in three young cats, the motion of the blood continued as before. The ſame thing happened to BLANQUET, in the experiments related by Dr. SENAC[x].

This made me reſolve to ſlit both the *cavæ*; I ſhould have cut them quite off, if I had not been afraid, that then the ceſſation of the motions of the heart, would have been attributed to the want of its neceſſary ſupporters. After having ſlit them, I emptied them of all their blood, and tied them. I next emptied the auricle; and then the ſucceſs of the experiment was

[s] Anat. p. 379. [t] De nat. hum. p. 62, 63. 306. See likewiſe D. SORGELOOS de œconom. corp. 66. 69.
[u] L. C. p. 99. [x] Trait. du Cœur t. 1. p. 449.

was always conſtant. As ſoon as I had freed the auricle of all its blood, and prevented it from receiving any afreſh, immediately its motion entirely ceaſed. As it is more difficult to empty the ventricle than the auricle, and as it yields to the impreſſions which are communicated to it by the left ventricle, I ſometimes obſerved there a gentle motion, but incomparably weaker than that, which it has when it receives the blood from its auricle and the *venæ cavæ*.

But there ſtill remained a more authentic experiment for me to make. In the natural ſtate, the right ventricle moves longer than the left; becauſe, as I ſaid before, it receives the veinous blood longer than the other. To prove to a demonſtration that the blood is actually the cauſe of the heart's motion, there is nothing farther required to be ſhewn, than that if the right ventricle and its auricle are deprived of the blood, while the left ventricle is not, the firſt immediately loſes its motion, while the other ſtill continues to act.

In order to ſucceed in this experiment, the right ventricle muſt firſt be perfectly emptied, by opening the pulmonary artery and *venæ cavæ*, and the emptying of the left ventricle prevented, by tying the *aorta*; and after this is done, we muſt examine narrowly, if the motions of the right ventricle ceaſe, and if the left and its auricle continue theirs.

After ſome attempts, which the difficulty of ſo delicate an undertaking, and the ſudden death

of the animals rendered fruitleſs, the experiment at laſt ſucceeded to my wiſh; *viz.* the right auricle remained perfectly quiet, and its ventricle preſerved no other motion than that which was a neceſſary conſequence of the connection of its fibres with thoſe of the left ventricle, and which pulled the external *parietes* towards that which ſeparates the two ventricles from each other. The left auricle preſerved its motion for a certain time, the left ventricle ſtill longer; and I have ſometimes obſerved this laſt contract two hours after the other had ceaſed.

When the experiments ſucceeded exactly, the blood mounted from the point to the baſis of the left ventricle, and deſcended again from the baſis to the point, and then the left ventricle, as long as it preſerved any motion, appeared likewiſe to deſcend. At other times, as I have ſeen in a kid, it had no motion at all. This experiment ſucceeded beſt when the left auricle emptied itſelf freely into the ventricle, and the paſſage of the blood from the ventricle into the *aorta*, which was tied, was intirely ſtopt. The point of the left ventricle was always the part which preſerved its motion the longeſt. By this means the property of being the laſt living part of the body, is transferred from the right ventricle to the left, by preſerving longer in the laſt, the irritation produced by the contact of the blood.

A new force may be given to this experiment, by trying to blow into the right ventricle; by which irritation its pulſations are renewed.

Farther, I have always remarked, that the internal ſurface of the heart is much more irritable than the external. For when I irritated this laſt with the ſtrongeſt poiſons, the motion thereby communicated to the heart ſoon ceaſed; whereas the irritation communicated to the internal ſurface, by means of air alone, has occaſioned, eſpecially in frogs, and even in cats, motions which ſubſiſted a very long time, even after all the parts were quite cold.

I have repeated this laſt experiment nine times, *viz.* of preſerving the motion of the left ventricle, after all the other parts were deprived of theirs; ſeven times upon cats, and twice on kids. The violent ſtruggling and agitation of dogs, render them improper ſubjects for this purpoſe.

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